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Newton's "Proof" of the Sine Law and his Mathematical Principles of Colors

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Communicated by J.E. HOFMANN

NEWTON'S inaugural lecture of January, 1670, in Cambridge is an attack on the received laws of refraction: "In refraction I find an irregularity that disturbs everything." In the *New Theory about Light and Colors* (February, 1672) he estima-

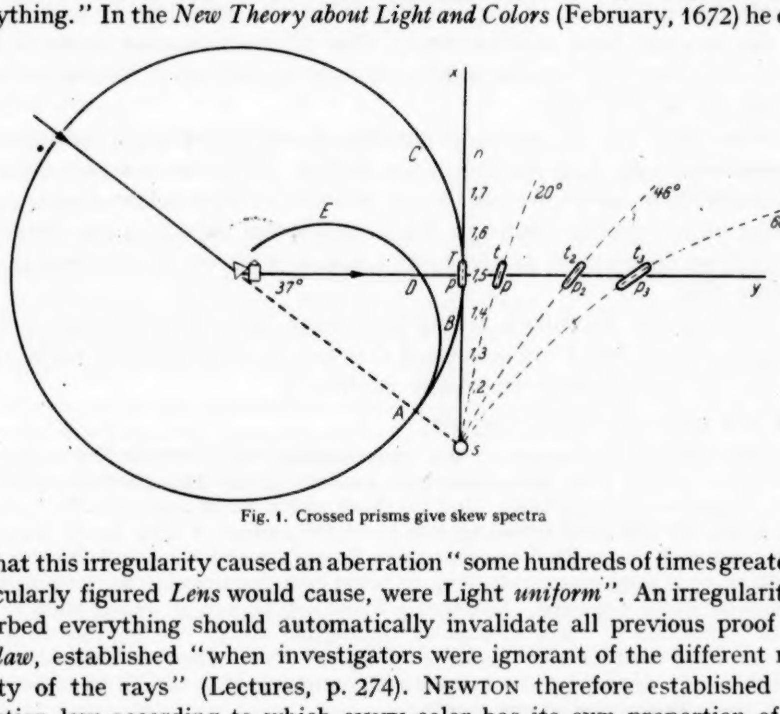

Fig. 1. Crossed prisms give skew spectra

ted that this irregularity caused an aberration "some hundreds of times greater then a circularly figured *Lens* would cause, were Light *uniform*". An irregularity that disturbed everything should automatically invalidate all previous proof of *the sine law*, established "when investigators were ignorant of the different refrangibility of the rays" (Lectures, p. 274). NEWTON therefore established a new refraction law according to which every color has its own proportion of sines. This amended law ought to be verified experimentally, and NEWTON derived his test from the spectra cast on the wall by crossed prisms.

NEWTON'S description of this experiment is not so detailed as might be wished. Therefore I shall first give my own conception of it. Let us look at Fig. 1, where a sunbeam proceeds to the center of a spherical chamber. There the beam is deflected by a prism and dispersed into a circular arc ABC, but we perceive only the visible part TP of the spectrum. For theoretical reasons we wish equal

refractions on both sides of the prism, so we rotate it slightly as we determine the different parts of the spectrum. We then take another identical prism and place it in cross-position immediately behind the first. The spectrum on the spherical wall will now follow the curve ADE, whose projection onto the rear wall is *a circle*. For each separate index of refraction we must ensure equal refractions also for the second prism before we mark out where the corresponding ray impinges on the wall. NEWTON, who considered only a very small spectral range, did not bother about this but provided symmetry only when the index of refraction was about 1.54. Our spherical wall is a mathematical device to help us understand the spectral curve better. NEWTON of course cast the spectrum on a plane wall, which in our figure is situated to the right. I find for the spectral curve on that wall a parametric equation

$$x = R\,\mathrm{tg}\,(\varphi - 37°11'),$$

$$y = R\,\frac{\mathrm{tg}\,\varphi}{\cos(\varphi - 37°11')}.$$

NEWTON's first prism had an angle of 60°. When the second prism has the same angle, the spectral curve will be Sp_3t_3. But when the second prism is interchanged with others of smaller angles, the curves will be as indicated on the figure (for 46° and for 20°).

NEWTON, who did not know of infrared or ultraviolet rays, only observed small segments (t_3p_3, t_2p_2 and tp) of the curves. When we compare these and other intermediate curves, we can obtain, at least in theory, a law of refraction for each color of the visible spectrum. But it is not our object to investigate this possibility; we concentrate on the curve t_3p_3 and on NEWTON's statements about this curve only.

In small details NEWTON's arrangement may have been different from the one proposed here, but I am convinced that any small differences will be immaterial, leaving our argument essentially unaffected.

We now quote NEWTON's *Opticks:*

The late Writers in Opticks ... not understanding the different Refrangibility of several Rays ... from their Measures we may conclude only that the Rays which have a mean Degree of Refrangibility, that is, those which when separated from the rest appear green, are refracted according to a given Proportion of their Sines. And therefore we are now to shew, that the like given Proportions obtain in all the rest. That it should be so is very reasonable, Nature being ever conformable to her self; but an experimental Proof is desired. And such a Proof will be had, if we can shew that the Sines of Refraction of Rays differently refrangible are to one another in a given Proportion when their Sines of Incidence are equal... Now, when the Sines of Incidence are equal, it will appear by the following Experiment, that the Sines of Refraction are in a given Proportion to one another.

(NEWTON then says that the direct sun beam paints the image S on the wall, but when the beam is deflected by the first prism, it paints the spectrum TP *etc.*)

And when the Refraction of the second Prism was equal to the Refraction of the first, the refracting Angles of them both being about 60 Degrees, the Axis of the Spectrum p_3t_3 made by that Refraction, did when produced pass also through the middle of the same white round Image S. But when the Refraction of the second Prism was less than that of the first, the produced Axes of the Spectrums tp or t_2p_2 made by that Refraction did cut the produced Axis of the Spectrum TP in the points m and n, a little beyond the Center of that white round Image S ...

... the Proportions of the Sines being derived, they come out equal, so far as by viewing the Spectrums, and using some mathematical Reasoning I could estimate. For I did not make an accurate Computation. So then the Proposition holds true in every Ray apart, so far as appears by Experiment.

Opticks, Book I, Prop. VI

NEWTON at times shocks his students by conclusions as reckless as these. Let us recapitulate:

A. Although NEWTON rejects the old law of refraction, he grants his predecessors that they proved the proportion of sines for mean refrangible rays, because "'tis to be presumed that they adapted their Measures to the middle of the refracted Light".

B. He depicts the skew spectra as in Fig. 2, where the spectrum t_3p_3 points directly toward *S*.

C. NEWTON says that when the proportions of sines are derived from the skew spectra on Fig. 2, these proportions "come out equal".

I do not stop to criticise NEWTON'S inference (A), because the objections to the other two are much more serious.

B. NEWTON'S diagram is erroneous. The spectra should have been as in Fig. 1. The spectrum t_3p_3 does not point toward *S*.

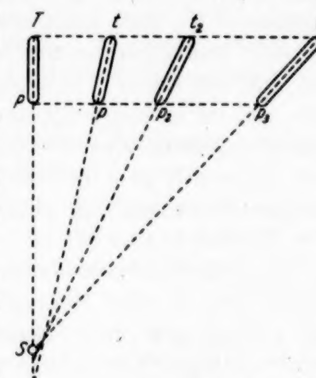

Fig. 2. The skew spectra from NEWTON'S *Opticks*

C. The proportions cannot come out equal from NEWTON'S diagram.

Seldom has a physical law been "demonstrated" by experiments so inaccurate and by deductions so faulty. But what of it? There is such inner consistency in the circumstantial evidence presented elsewhere in the *Opticks* by NEWTON that most of us are convinced even without direct proof. What is at stake?

Tis ye truth of my experiments which is ye business in hand. On this my Theory depends, & which is of more consequence, ye credit of my being wary, accurate and faithfull in ye reports I have made or shall make of experiments in any subject, seeing yt a trip in any one will bring all ye rest into suspicion.

NEWTON to OLDENBURG, 28 November 1676

I do not wish to throw suspicion on the rest of NEWTON'S prism experiments. He was ordinarily very painstaking and accurate, and among his few "trips" we have here selected the worst. After the first years of opposition no person of note, except GOETHE and SCHOPENHAUER, seriously challenged the correctness of NEWTON'S experiments on colors, except in details. Even NEWTON'S greatest antagonist said about the general result of the experiments:

Vix tamen ausim credere NEWTONUM hic labi, in re tam capitali, & quam tanto studio excussit.

LEIBNIZ to BERNOULLI, 15 October 1710

EULER, who is very skeptical about NEWTON'S theories of light, does not discredit his experiments with prisms.

For my part, I set about the study of NEWTON'S manuscripts and books without any preconceived notion that he was impeccable. But I admit to a great

admiration for his experimental skill, an admiration which steadily increased as I found I could not imitate his prismatic experiments with even approximately the same success as he had.

But how could NEWTON fail so sadly in the fundamental experiment we have analysed?

NEWTON usually let the rays suffer equal refractions on both sides of the prism. Small deviations from this ideal position are not critical. But with *crossed* prisms one has to be very careful. The edge of the second prism must be kept in exact cross-position to the first prism. A small deviation here, and the direction of the skew spectrum is sensibly altered. Probably NEWTON erred here. When he was experimenting with more than one prism he usually fastened the first, but not always the second. Maybe he made this experiment in his youth only, and rather hurriedly, and then in his old age hit on the idea that the skew spectra might serve, even in practice, to determine a law of refraction. While he was then apparently past the time when he could make delicate experiments, he could and should have worked out conscientiously the theoretical aspects of the problem. It is difficult to understand how he could believe his figure (2) to be correct.

Few historians have examined in detail NEWTON'S experiments and statements. BREWSTER and other biographers fashioned and painted NEWTON as a superman and a knight *sans erreurs et sans reproche*. Modern man, who is skeptical and disbelieves in legends, soon tires when he must look at a statue that in all illumination casts no shadows. NEWTON can captivate the minds of our age only if his biographers admit that also in him there is an interplay of light and shadows, and only so will his true greatness emerge, against a background of considerable weaknesses and mistakes.

Having now let our searchlight rest on NEWTON'S handling of a central law, we shall let it sweep more swiftly over other parts of NEWTON'S optics and especially over the mathematics there. We shall watch the rise and decline of NEWTON'S "mathematical science of colours". This science, belonging to rational mechanics, prepared NEWTON for the celestial dynamics of his *Principia*.

The leading physical ideas and principles of the young NEWTON were deeply influenced by DESCARTES. From him NEWTON took over the *subtile matter* that filled the so-called *vacuum* and penetrated into the pores of all dense bodies. The *ether* was made up of this subtile matter, which was interspersed with light globuli and a multitude of other small corpuscles. It is important to note that NEWTON'S ether was rarer in the denser bodies, so that the light globuli moved faster there than in air or vacuum.

According to DESCARTES the light globuli did not really move but only showed an *inclination* to move. DESCARTES considered light as a *pressure*. But when he deduced the laws of reflection and refraction DESCARTES treated light rays as if they were *moving* globuli. They met less resistance in the denser bodies than in air. NEWTON took a step from which DESCARTES had refrained. The globuli really moved, he said. While DESCARTES had accounted for the colors by attributing *different rotations* to the globuli, NEWTON said that they had *different linear velocities*, the red-making rays being swifter than the blue-making.

But not to digress too much on similarities and differences between DESCARTES and NEWTON, we state without introduction some principles to which

NEWTON adhered during the first half of his long stay in Cambridge. We select principles concerning the ether and the light globuli:

1. There is everywhere a subtile matter that resists the motion of the light globuli and of other corpuscles.

2. The ether is rarer in the denser bodies. Therefore the light globuli move faster there.

3. When a globulus penetrates into a denser body, its tangential velocity is unchanged, but the total velocity increases and is independent of the angle of incidence.

4. In vacuum the red-making rays move faster than the blue-making rays. And according to the preceding principle, the red rays will move more swiftly than the blue in every transparent substance.

5. There are no "jumps" in the operations of nature. All gradations of velocities exist and also all intermediate sizes of corpuscles.

6. "Each body can be transformed into any other and take on successively all the intermediate gradations of qualities."

Principia, 1st edition, p. 402

7. "For nature is a perpetuall circulatory worker."

Hypothesis of 1675 (Corresp. I, p. 366)

NEWTON did not openly admit that he followed such principles, but they are very apparent in his manuscripts, and, when we look for them, also in his printed works. I cannot give the complete documentary evidence in this article, but I shall go rapidly through the source material, published and unpublished.

Among the many interests which the young NEWTON reveals in his earliest notebooks, we perceive his predilection for colors and color mixtures. His first notebook (Greenstreet, pp. 16–25) contains recipes for painters' colors and printers' inks and also many experiments in the mixing of colors. In his boyhood NEWTON took an especial pleasure in tricks, such as changing water to wine. But little in this early notebook points toward the future scientist; we find no rules for mixing colors, and of course no mathematical treatment of the colors.

In a second notebook (Add. 3996) NEWTON, now a student in Cambridge, still prepares, changes and mixes colors, no longer colored powders or liquids, but the phantasmal colors seen in a glass prism or painted on the walls by sun rays refracted by a prism. The prismatic colors seemed to obey constant and general rules. NEWTON all his life hoped for a comprehensive formula or prescription applicable for all color mixtures.

In this notebook NEWTON also records his first systematic scientific investigation: He tabulates the colored bands that illuminated bodies exhibit when they are looked at through a prism. These bands appear only in the boundary between bodies of different colors or different intensities of the same color. *White* above *black* gives a streak of red and yellow, the most impressive of the bands.

Earlier physicists had observed the bands, and it will be instructive to compare their different attitudes to this observation:

1. WITELO (1225–1280?) compiled from the optics of ALHAZEN, PTOLEMY and ARISTOTLE a textbook that was studied by all physicists until the middle

of the 17th century. WITELO knew of the hexagonal crystal and the new colors that appeared when he held it before his eye:

And if you revolve the crystal before the eye, many diversities will occur, and someone might apply these to the generation of colors. For the color or the visible form is not conveyed to the eye unless by the nature of light that is in it. A diligent inquisitor might by experience add much to what has been said here.

Perspectiva, p. 474

2. The inquiry that WITELO had left undone was soon taken up by THEODERIC OF FREIBERG († 1311). He held the prism before his eye and wondered where the colors came from. For the object was not colored, nor was the prism. The medium (air) and the eye were also devoid of the colors he saw. After many arguments he concluded that the new colors were *in the prism*, but only in the peculiar way in which an image is in a mirror.

THEODERIC was a child of his age, often referring to ARISTOTLE, but unlike his contemporaries he was an experimentalist of no mean abilities. He placed his prisms, his glass globes and his spherical water flasks at different angles to the sunbeam and to the eye. He found rules for *when*, *where* and *in what order* the four principal colors appeared. But when he tried to measure, he got impossible results, for example, a rainbow radius of only 22 degrees.

3. Three hundred years later the importance of accurate measurements was realized, and especially by the Englishman THOMAS HARRIOTT (1560—1621). HARRIOTT measured whatever could be measured and also the red and yellow bands to be seen through the prism. In a diagram he delineated the red, yellow and green rays as they passed through the prism and to the eye. He measured angles and lengths, and he computed the indices of refraction. He did not limit his trials to solid glass prisms but used also hollow prisms which contained different liquids: rain water, salt water, spirits and turpentine. He believed that he had found a universal law for the color dispersion.

If the mean and extreme indices of refraction are called n_0 and n_e respectively and the angle of refraction for grazing incidence is called r_m, HARRIOTT stated his refraction and dispersion laws thus (my notations, not HARRIOTT'S):

$$\frac{\sin r_0}{\sin i} = \frac{1}{n_0} = \sin r_{0m} \quad \text{and} \quad \frac{\sin r_e}{\sin i} = \frac{1}{n_e} = \sin(r_{0m} - 9').$$

Unfortunately, because of insufficient explanation, we cannot be sure if HARRIOTT meant the extreme red or the extreme blue rays when he established his law for n_e. HARRIOTT'S dispersion law is experimental and is supposed to apply to all the five substances he investigated.

4. Sixty years later the young NEWTON also observed the colored fringes seen through a prism, and he drew a diagram that is surprisingly like HARRIOTT'S, but NEWTON did not measure the colored bands or compute any index of refraction from them. Instead he systematically contrasted objects of new colors, and set down the new bands he saw in the prism when red and blue were contrasted to black or white.

Instead of measuring indices of refraction NEWTON in his first years of research studied DESCARTES and amended DESCARTES' hypothesis of light and colors.

NEWTON preferred to consider the light rays as streams of globuli and assumed that different colors were impressed on the retina according to the sizes and velocities of the globuli. At that time the correct theory of collisions was understood only by HUYGENS, who kept it to himself, but all the same NEWTON tried to compute how much light globuli were retarded when they impinged upon loose particles in the pores of bodies.

Parallel to the second notebook NEWTON also used a commonplace book (Add. 4000) where he recorded speculations and more or less systematic observations. There we find a chapter *Of refractions*, where NEWTON proposed and described instruments for grinding lenses without spherical aberration. We infer that he had not yet (1665 ?) discovered that the colored bands were a more serious obstacle to the perfection of telescopes than an incorrect spherical figure.

Another of NEWTON's notebooks, a very small and thin one, is now in the possession of the Fitzwilliam Museum in Cambridge. Here NEWTON has inserted a small treatise on conics. Its 40 problems and theorems are elementary and apparently gathered from the study of SCHOOTEN's *Organica Descriptio* (1646) and MYDORGE's *Prodromus Dioptricorum* (1631). NEWTON shows no originality in his little treatise, unless perhaps in the third problem where he invents a false method for describing an ellipse. About the same time NEWTON had made great contributions to the squaring of areas by WALLIS' methods, and he was very near to the generalized binomial formula. NEWTON's mathematical education certainly did not proceed along traditional lines.

Few entries in NEWTON's notebooks can be dated exactly, although they also contain astronomical observations. When NEWTON began a new notebook he at once provided the first part of it with headings that indicated the question he intended to treat there. This procedure resulted in many blank pages, while for other topics the allotted space proved too small. Thus under the heading *Of Colours* we find a series of notes and observations that swelled into a small treatise, interrupted by previous entries and therefore continuing on empty pages in the latter half of the notebook. Henceforward we shall call this treatise *Colours I* to distinguish it from a later treatise which we shall call *Colours II*.

NEWTON made his last entry in a notebook many years later than the first, and the chronology is difficult. We mention a very interesting table of dispersion on the verso of fol. 33 in Ms Add. 4000. This table cannot be earlier than 1669, but it comes immediately after NEWTON's chapter (1665 ?) on the grinding of lenses.

The treatise *Colours II* occurs in a rather big notebook (Add. 3975) which was in use until NEWTON had finished his *Principia*. *Colours II* has 64 paragraphs and embodies material from *Colours I*, but in addition there are many observations of "NEWTON's rings", which had been described first by HOOKE. However, our article is chiefly concerned with the sun spectrum, NEWTON's first description of which is recorded in paragraphs 7 and 8 in *Colours II*. There we also have NEWTON's first computation of the indices of refraction for blue and red rays.

It is well worth noting that here (in 1668 ?) NEWTON measured a spectrum whose length was 3 or $3\frac{1}{2}$ times the breadth. But after 1670 he stubbornly

insisted that the length must be at least 4 or 5 times the breadth, although the angles of the prisms were all nearly 60 degrees.

We shall compare his first indices of refraction for glass with later results:

	Colours II 1668?	Lectures 1670	Opticks 1704
Blue rays .	1.50646	1.5593	1.5533
Red rays .	1.49984	1.5367	1.5413
Dispersion.	0.0066	0.0226	0.0120

Although the numbers from 1670 refer to the *extreme* red and blue and the others to the *most brilliant* red and blue, the chromatic aberration is decidedly less in 1668 than later. The mean index of refraction was then about 1.50, whereas his later prisms had about 1.55. In 1675 NEWTON declined to believe in the numbers of his opponent LUCAS, although they differed little from his own numbers from 1668. We shall try to clear up this mystery later.

Beyond the informative notebooks there is much optical material on loose sheets, especially in the bulky manuscript Add. 3970. But we proceed to the printed sources. NEWTON's optical *Lectures* (Lectiones) *I* and *II* were given in Cambridge 1670—71. They remained unprinted till 1728, a year after NEWTON's death. He would probably never have consented to publish *Lectures I* as they were originally professed.

In later life NEWTON was inveigled in a controversy about the shape of the earth, whether the globe is *oblong* or *flattened*. In NEWTON's optical polemics a similar question was raised about the sun's image, when cast on a wall by a prism. In *Lectures I* NEWTON demonstrated that with *uniform rays* this solar image should be circular or slightly *flattened*. NEWTON's handling of this proof is characteristic as showing us amongst other things how he tried to evade difficulties. We shall therefore examine it in some detail:

1. He imagines a symmetrical position of the prism. In this position the sun rays suffer the same refraction at the entrance as when they leave the prism.

2. As only one single ray can be exactly symmetrical, it seems convenient to me to assign symmetry to the ray from the sun's center. But NEWTON says that the rays from the sun's upper rim shall deviate from the ideal symmetry exactly by the same angle as the rays from the lower rim. The ray from the sun's center cannot be exactly symmetrical in this arrangement.

3. In his proof NEWTON supposes the prism to be on the *outside* of the hole through which the beam enters. This is peculiar, for in the experiment NEWTON puts the prism on the *inside*.

4. NEWTON applies only pure geometry in his proof. His predecessor HARRIOTT had calculated the deviations numerically.

I think such peculiarities very informative about NEWTON, if they can be explained:

1. A symmetrical position of the prism simplifies enormously both the experiment and the theoretical deductions.

2. and *3.* I have investigated for myself the arrangement I consider natural, namely prism on the inside and exact symmetry for the ray from the sun's center. I found a slightly *oblong* or rather egg-shaped image of the sun. But if the prism is supposed to be on the outside, the solar image will be flattened, as NEWTON contends.

The issue is somewhat confused, because it was not necessary for NEWTON to resort to a trick here. When we look up the corresponding place in the *Opticks* (Fig. 13), we find the prism on the inside.

4. When we use only pure geometry it is very difficult to visualize in detail how and how much the skew rays deviate from symmetry. NEWTON's proof holds, as far as it goes, but it can scarcely have convinced the average student in Cambridge. For one of his assertions NEWTON gives no proof, "utpote nimis longam & proposito meo non omnino necessariam".

Apart from some experiments in the opening chapter, *Lectures I* are chiefly mathematical, for NEWTON wishes to show "that the Science of Colours is most properly a Mathematicall Science" (Corresp. I, p. 187). NEWTON evidently proposed to erect this mathematical science of colors on a dispersion law, which is as follows:

When r'_m and r''_m denote the refraction angles for *grazing* incidence, for the extreme blue and red rays, respectively, the refractions from air or vacuum into *any* transparent substance obeys the law

$$\frac{\operatorname{tg} r''_m}{\operatorname{tg} r'_m} = \frac{40}{39}.$$

NEWTON does not deduce this law in his Lectures. Instead we find an unusually frank admission in the most astounding passage NEWTON ever wrote in his scientific works:

> I have not yet verified this theorem by experiments. But it seems to me that it can scarcely differ much from truth, and I have therefore no scruples in assuming it without proof for the present. Later I may confirm it by experiments; or if I find it erroneous, I shall correct it (Opera III, pp. 288—289).

If we feel uncomfortable after this, we are dumbfounded when we understand that this theorem is the pillar on which NEWTON tries to build a heavy mathematical superstructure. And naturally we ask why NEWTON was so convinced of the truth of a theorem that we know to be false.

The answer is that he could deduce it from his preconceived principles (2, 3 and 4). The tangential velocity (XE) of the red-making rays is unchanged by the refraction, and so is the tangential velocity (XC) of the blue-making rays. But all rays acquire *equal* normal velocity ($ET = CP$). From his measurements with glass (Add. 4000, f 33 v) NEWTON had found that the proportion XE/XC was 40.4/39.4, which is altered to $\frac{40}{39}$ in his *Lectures*.

NEWTON's successful extrapolations in mathematics may have made him reckless. He relied too much on his secret principles and on his conceptual image of light rays as streams of globuli. From these he deduced his sine proportions, and they enabled him to plan a superfluity of impressive qualitative experiments with prismatic colors. The success of the latter gave him such confidence in the principles that he dared to assert his dispersion law without sufficient experiments.

The proportion $\frac{40}{39}$ had been measured for one sort of glass, but NEWTON believed in the same proportion for all glasses and even for all transparent substances!

We come to the third and most curious section of *Lectures I*. To account for it we must suppose not only preconceived opinions in NEWTON, but also loosely founded expectations and hopes. It seems to me that NEWTON went to his first course of University Lectures intending to present a totally new mathematical science, although he had not yet worked it out completely. At least, he had not conscientiously deduced the implications of his dispersion law.

Belated exertions do not always result in the expected success, not even with NEWTON. But at first he made some progress. He let the blue and red rays of Fig. 3 be refracted by further parallel surfaces. According to his dispersion law the homocentric pencil of diversely colored rays should be concentric still, but the center of the pencil jumped up and down the perpendicular FG each time the rays were refracted.

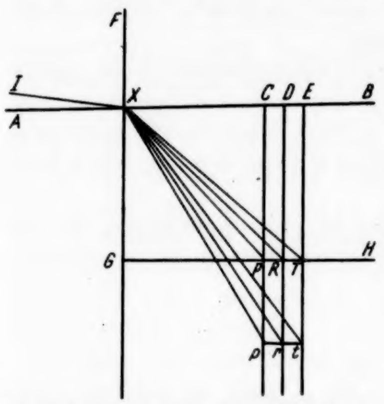

Fig. 3. NEWTON's diagram for his dispersion law

But if NEWTON had expected a similar property for all possible refractions, he was sorely disappointed. NEWTON's grip on the field of inquiry loosens. From direct statements and formulae suitable for numerical computations, NEWTON turns to inequalities and things of comparatively little consequence, which fill the middle part of *Lectures I*. Again and again NEWTON seems to rally his resources in order to emerge from auxiliary theorems and barren inequalities. But we are disappointed. The magnificent geometrical superstructure that NEWTON must have dreamt of was never completed. NEWTON therefore turned to the familiar domain of geometrical optics for *uniform* rays and made notable contributions here. But only in the two last pages did he return to the colored rays, now considering their passage through a convex lens. He computed the error occasioned by different refrangibility to be 1521 times the error due to spherical aberration. As HUYGENS pointed out, NEWTON vastly overestimated the inconvenience of chromatic aberration.

In the second part of his lectures NEWTON wisely omits optical mathematics and concentrates on his observations of colors. Roughly we can divide his extremely dexterous experiments into two groups. To the first belong experiments where NEWTON re-examined all previous refraction experiments under conditions where he inferred that colors would be discernable. This was, according to his principles, whenever the refractions were very great or cumulative. Thus he studied anew the total reflection and verified that in the neighborhood of the limiting angle r_m red rays might be transmitted where the blue rays of the same beam suffered total reflection.

When a white sunbeam enters a spherical flask of water and is reflected once or twice before it emerges again, the ray ought to be split into colors. This experiment is important, because the flask represents a raindrop. NEWTON completed the classical theory for the rainbow by explaining the colors and

computing the breadth of the two bows. He deduced that colors should appear not only by splitting the rays in prisms and globes, but also in plane-parallel glass plates. When he used very oblique incidences, he found this to be as expected. Moreover, the red and blue rays should have different foci when they are assembled by a convex lens. NEWTON verified this. His heuristic principles seemed triumphant in nearly all cases. But he may have felt, in spite of all apparent assurance, some discomfiture when he proclaimed that also the famous color phenomena of the infusion of *Lignum Nephriticum* could be explained by his theories. The most impressive phenomena in this infusion are caused by fluorescence.

In a second group of experiments NEWTON returned to his boyhood delight in color mixtures. By various contrivances he separated or isolated the colors of the split ray. Then he brought two or more colors together again.

But in all this abundance of observations and experiments we still miss an experimental determination of refractions for the separate colors, and of course NEWTON knew that something was lacking.

NEWTON's optics are so stimulating because he often does the unexpected. Instead of conscientiously measuring refractions for each color he proposes a quite extraordinary rule for the division of the solar spectrum. This division corresponds to the well known musical division of the monochord. NEWTON put 360 for the total length of the string. In his time the scale was sounded by striking the string lengths:

360 320 300 270 240 216 $202\frac{1}{2}$ 180

violet indigo blue green yellow orange red

NEWTON's early notebooks tell us that he was very interested in the theory of musical sounds and consonances. He wondered whether the numerical proportions of the monochord were not to be found again somehow in the sun spectrum. We might suspect NEWTON of being influenced by the celestial harmonies of KEPLER's *Harmonices mundi liber V*, were it not that NEWTON seems to have had small direct knowledge of KEPLER's works, with the sole exception of the *Paralipomena* and perhaps the *Dioptrice*.

Anyhow, NEWTON with an assistant marked out on the sun spectrum the confines between the colors and found, as indicated above, the same divisions as on the string (*i.e.*, on one half of it). He says in his *Hypothesis of 1675:*

> And further, as the harmony & discord of Sounds proceed from the proportions of the aereall vibrations; so may the harmony of some colours, as of Golden and blew, & the discord of others, as of red and blew proceed from the proportions of the æthereall. And possibly colours may be distinguisht into its principall Degrees, Red, Orange, Yellow, Green, Blew, Indigo and deep violett, on the same ground, that Sound within an eighth is graduated into tones.

With this musical division of the spectrum and with his dispersion law NEWTON could attribute to each color a definite index of refraction. Wherever the mean index of refraction was known for a substance, his theories permitted him to compute the indices for all other colors.

In the first months of 1670 NEWTON's expectations must have soared very high, perhaps higher than at any other time in his long life. If his dreams had come true, the mathematical science of colors would have eclipsed even his

Principia. For there he relies on KEPLER's laws and on observations of the astronomers, but the mathematical science of colors would have been completely his own, for NEWTON had done all the experimental work himself.

But if this science partly collapsed before it had attained the heights of his dreams, there was still left so much that he could write to the Royal Society and announce

> the oddest if not the most considerable detection wch hath hitherto beene made in the operations of Nature.
>
> (January 1672)

If NEWTON's "odd detection" made no stir in Cambridge, it was the more applauded by the Royal Society. But this society got a treatise very different from the lectures NEWTON gave his students in Cambridge. His new treatise, extremely well planned and executed, is perhaps the greatest masterpiece in the history of optics, rivalled only by HUYGENS' *Traité de la Lumière.* But we must not believe that NEWTON gave a true "*historicall Narration*" of his optical discoveries.

His treatise was speedily printed, and NEWTON advertised for control experiments and asked for a discussion of the theory. He got both. But it turned out that NEWTON expected and wished only unanimous approval. He was irritated and at times incensed by criticism. He retorted to HUYGENS' objections so insolently that HUYGENS would not dispute any longer, and after HOOKE's remarks, in many respects justified, NEWTON threatened to withdraw from the Royal Society. He was duly gratified when Father PARDIES surrendered, but this was his only victory apart from the approval of the Royal Society. In the French Academy MARIOTTE could not verify NEWTON's experiments. HUYGENS and HOOKE were both surprised and disgusted with his attitude, and NEWTON was at last in contact only with second-rate scientists from the Roman Catholic college in Liège. There LUCAS measured the sun spectrum and found it considerably shorter than NEWTON had announced. NEWTON flatly denied the truth of LUCAS' result. He was apparently still very sure of the dispersion law, which was a secret to most, although he had presented it in his Cambridge lectures.

But much of his secret laws and fundamental conceptions were divulged in an "Hypothesis" NEWTON in 1675 sent to the Royal Society. This *Hypothesis* or second treatise on light and colors was necessitated by an apparent deficiency in NEWTON's first treatise. This had neither explained the inflection of light, discovered by GRIMALDI, nor the colors of thin plates, described by HOOKE. To account for these phenomena NEWTON in his *Hypothesis* amends his previous theory of light by introducing vibrations of the ether. All denser bodies are enveloped in "rigid" ether surfaces from whence the light globuli are reflected. NEWTON expressly points out that the globuli are not reflected by collision with the solid body. Also refraction is caused by the ether surface which has properties similar to the surface membranes of liquids. The light globuli keep a constant velocity in the resistant ether because they are

> continually urged forward by a Principle of motion, wch in the beginning accelerates them, till the resistance of the Ethereall Medium equal the force of that principle, much after the manner that bodies let fall in water are accelerated till the resistance

of the water equalls the force of gravity. God who gave Animals self motion beyond our understanding is without doubt able to implant other principles of motion in bodies wch we may understand as little. Some would readily grant this may be a Spiritual one; yet a mechanical one might be showne, did not I think it better to passe it by.

After this cryptic remark NEWTON explains how the paths of the globuli are incurvated in the rigid ether surface. The globuli are first retarded, but immediately afterwards accelerated to an increased velocity by the motive principle they possess.

NEWTON at one time believed that the globuli had an inherent periodicity, but this assumption was abandoned in his official hypothesis, where the globuli excited vibrations in the ether only when they impinged upon one of the rigid ether surfaces. These vibrations outran the globuli that had excited them and arrived first at the other surface of the thin plate. This other surface was thereby periodically dilated and compressed, and all globuli that arrived at it when in a state of compression were reflected, otherwise transmitted.

Inflection of light was likewise explained by the rigid ether surfaces. In this connection we mention that NEWTON imagined gravity of bodies occasioned by streams in the ether directed towards the centers of the sun and the planets. And the ether with its particles played an indispensable part in his alchemical theories and particularly in his alchemical cosmogony. He imagined descending and ascending streams of different ether constituents which are fixed in the bowels of the earth and there sublimate after a long time and ascend again. The light globuli are not essentially different from other particles that move in the subtile matter of the ether. "Are not gross Bodies and Light convertible into one another?" NEWTON still asks in his 30th query. Perhaps NEWTON tended to regard our planetary system with its interplanetary space as a gigantic alembic or distillation apparatus for that perpetual circulatory worker, Nature.

In 1680, after HOOKE had asked NEWTON to calculate the orbit of a planet, NEWTON's cosmogony and physical principles changed radically. HOOKE had proposed these principles:

If all external "impressesses" were removed, the planetary body would continue in uniform motion along a tangent to its orbit.

The actual path of the planet is caused by a "supervening attractive principle" which incurvates this straight motion. This principle causes an "endeavour" to the sun, which varies as $1/r^2$, r being the distance from sun to the planet.

NEWTON did not manage to solve HOOKE's problem directly, but he mastered the inverse problem. On the basis of KEPLER's first two laws he found an acceleration proportional to $1/r^2$ and directed to the sun.

As a young man NEWTON had made similar computations for the moon's orbit, which he considered as a circle, so as to simplify computations. But his early calculations were rather half-hearted and not pursued further, for apparently he did not attach much importance to the first results. He believed in a resisting ether that would retard the motion along the tangent. Therefore we can imagine his bewildered astonishment when his calculations from 1680 onwards showed that the subtile matter with all its particles offered no appreciable resistance to the planets. And it must have been with very mixed feelings that

he gradually realized that he had made a fundamental discovery in planetary theory. For this discovery overthrew the doctrine of a resistant ether and with it also NEWTON'S alchemical cosmogony and essential parts of his theory of light and colors.

But NEWTON rallied, to all appearances at least, also from this debacle, and as this failure in 1670 had been converted into a victory, he now composed the *Principia*, hailed as the greatest work ever produced by human intellect.

We are here concerned not with the *Principia* as a whole, but only with its optics, at the end of Book I. The changes in NEWTON'S optical theories are very apparent. The dense bodies are no longer enveloped in rigid ether surfaces, for the function of these is taken over by attractive force fields of very short range. His light globuli no longer have inner motive power; they keep their velocity in vacuum because of their inertia. NEWTON now wisely refrains from saying why they also keep a constant velocity in the dense bodies, and how it can be that light globuli are reflected uniformly when they come from the outside. The attractive force fields accelerate the transmitted globuli to an increased velocity. NEWTON was now able to determine the shape of the very short curved path when the globulus was traversing a refracting surface. For a uniform field it was a segment of a parabola, and otherwise the curve was composed of small parabolic arcs.

From these conceptions it was easy to deduce DESCARTES' law of refraction:

$$\frac{\sin i}{\sin r} = \frac{v_r}{v_i}.$$

NEWTON'S deduction is in many respects superior to DESCARTES', but it is difficult to imagine how NEWTON'S globuli could follow a straight path in the sinuous pores or interstices within the transparent bodies. LEONHARD EULER has pointed out that although NEWTON in the *Principia* abolished or ignored his previous resisting ether, it was of no avail. For the immense multitudes of his light globuli traversed the interplanetary spaces with immense speed and would still impede the motion of the planets.

Parts of the *Principia* are polemical, against the vortices of DESCARTES, and against the wave theory of light. The latter is attacked in Sect. VIII of Book II.

Prop. XLI: Pressure in a fluid substance is not propagated along straight lines, unless where the particles of the fluid lie in a straight line behind each other. p. 354

Prop. XLII: Every motion propagated in a fluid substance diverges from the straight path and into unmoved spaces. p. 356

Prop. XLIII, Corollary: Therefore they are mistaken who believe that the agitation of the parts of the flame conduces to a pressure in the ambient medium along straight lines. Such a pressure should be caused not by the unordered agitation of the parts of the flame, but by a swelling of the whole. p. 360

After the *Principia* NEWTON was incessantly urged to publish his optical papers; for the *Hypothesis* and his Cambridge lectures had not been printed, and therefore most of his prismatic experiments and his observations on colors in thin films were still unknown to all who did not consult the archives in the University of Cambridge and in the Royal Society. More than others JOHN WALLIS was forward in his admonitions to NEWTON about printing, for NEWTON'S own good and for the honor of the nation. Had not NEWTON'S doctrine of fluxions

been usurped, or so WALLIS argued, by others and circulated in the Netherlands as the differential calculus of LEIBNIZ? Would NEWTON take the risk of being deprived also of other discoveries?

NEWTON'S friends were not aware of his new dilemma, the greatest of all: *Ether or no ether*. Essential conceptions and the inner structure of NEWTON'S old optical theories had to be changed, and by a man whose head ached during his attacks on the theory of the moon. But overcome by the "importunities" of WALLIS and others NEWTON set to work and issued in 1704—not the comprehensive and perfect optics he once may have projected, but still a very great *Opticks*, where the unfinished investigations were bequeathed to others for further research (the queries).

It is generally believed that the *Opticks* is a compilation from NEWTON'S earlier works. But there are additions, such as a table of "refractive powers"; and NEWTON could not totally ignore the *Traité de la Lumière*, which HUYGENS had published in 1690. There HUYGENS, on principles very different from NEWTON'S, deduced as the law of refraction

$$\frac{\sin i}{\sin r} = \frac{v_i}{v_r},$$

a law recognised as true even by NEWTON'S friend HALLEY in a paper which he read before the Royal Society, March 1691.

In the *Opticks* (Book II, part III) NEWTON tries to find a relation between the density of a transparent body and its "refractive power", n^2-1. A relation of this kind seems natural if the refraction is caused by attraction from the surface atoms. Drafts (Add. 3970) to the *Opticks* show that NEWTON spent much time in composing a table of refractive powers and densities. He listed twenty substances and then concluded:

So then, by the foregoing Table, all Bodies seem to have their refractive Powers proportional to their Densities, (or very nearly;) excepting so far as they partake more or less of sulphurous oily Particles, and thereby have their refractive Power made greater or less.

... nothing more is requisite for producing all the Colours of natural Bodies, than the several sizes and densities of their transparent Particles.

HUYGENS explained very satisfactorily the irregular refraction of Iceland crystal, but he could not give any reason for another of its singular phenomena. He found that when two such crystals were placed in cross-position, the ray split in two by the first crystal was not split further in the second crystal. NEWTON tells us in one of his drafts (Add. 3970) that in his first trials (1669?) he had only one piece of this crystal and therefore could not possibly detect such things. In the *Opticks* (Quæres 25—29) he gives his own rule for the irregular refraction in calcite. I have not been able to find out or guess at the sense of this rule, which is erroneous.

NEWTON also makes some suggestions about the cause of the double refraction:

the unusual Refraction of Island-Crystal looks very much as if it were perform'd by some kind of attractive virtue lodged in certain Sides both of the Rays, and of the Particles of the Crystal. Quære 29

MACH finds that NEWTON

> with the happy manner, peculiar to his genius, of adapting his ideas to the realities of the case, summed up the problem with the question: "Annon radiorum luminis diversa sunt latera, diversis proprietatibus congenitis prædita?" a question which does, indeed, actually contain the solution to the problem.
>
> MACH: The Principles of Physical Optics, Chap. X

I cannot find that NEWTON *solved* the problem, nor do I see much credit in his *suggestion*. It is an old and easy device to invent additional properties and thus in a loose way to account for unforeseen experimental facts.

In his Hypothesis of 1675 NEWTON had invented "rigid" but compressible ether surfaces to account for the optical peculiarities of thin films. It would seem impossible for a man whose intellectual powers were declining to account for these phenomena now that he had abolished both the compressible surfaces and the ether that conveyed vibrations. But NEWTON had remedies, "Fits of easy Reflexion, and Fits of easy Transmission":

> Prop. XII. Every Ray of Light in its passage through any refracting Surface is put into a certain transient Constitution or State, which in the progress of the Ray returns at equal Intervals, and disposes the Ray at every return to be easily transmitted through the next refracting Surface, and between the returns to be easily reflected by it.
>
> What kind of action or disposition this is; Whether it consists in a circulating or a vibrating motion of the Ray, or of the Medium, or something else, I do not here enquire ...
>
> I content my self with the bare Discovery, that the Rays of Light are by some cause or other alternately disposed to be reflected or refracted for many vicissitudes.
>
> **Definition**
>
> The returns of the disposition of any Ray to be reflected I will call its *Fits of easy Reflexion*, and those of its disposition to be transmitted its *Fits of easy Transmission*, and the space it passes between every return and the next return, the *Interval of its Fits*.
>
> Prop. XIII. ... And probably it is put into such fits at its first emission from luminous Bodies, and continues in them during all its progress. For these fits are of a lasting nature, ...
>
> Book II, Part III

It seems that NEWTON is not quite consistent here, for in Prop. XII he says that the ray acquires its periodicity in the refracting surface, while later the ray already has this periodicity from its emission. But in spite of obvious objections to the "Fits" NEWTON in many respects is at his best in Book II. His technique of observations is supreme and his measurements better than in the prismatic experiments. We are perhaps too prone to translate his "intervals of their fits" with the modern "wavelengths", but we shall not forget that the wavelengths of the different colors can be computed from NEWTON's experimental figures for these "intervals", that is, if we use the modern theory for thin films. And it is only fair to say that NEWTON's conception of periodicity, although undeveloped, was superior to HOOKE's and that HUYGENS did not consider light periodic.

We note two further particulars in NEWTON's *Opticks:* His musical analogy for the sun spectrum recurs in several places. We admit that some sort of division

or mapping of the colors might be of value at a time when Fraunhofer Lines were unknown. But when the analogy appears also in the chapters on NEWTON's *rings*, we suspect that a convenient mapping was only part of NEWTON's design when he introduced it. New is a color disk, divided into seven colors according to the division of the musical scale. By means of this disk NEWTON gives a curious universal rule for what color will result by mixtures.

Thus the sixty year old NEWTON is back again to what possibly started his investigations, the problem of how to mix colors. We have let his colors and his theories pass before us, and it seems to me that they constitute a painting, or rather a panorama—a panorama of progress, of wild visions, of deep disappointments and of notable successes. This panorama, if I have obtained something of what I intended, is totally different from the conventional static picture in biographies and text-books. My inspiration has come from NEWTON's manuscripts and from reading between lines in his published works.

Some readers may think I have interpolated too much in gaps NEWTON left in his own narrative. But we know that NEWTON himself indulged very much in interpolations and extrapolations. He read between lines in the Book of Nature. At times he even chose to overlook some lines or to change a bit here and there, to make the whole conform better with his own notions.

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The optical manuscripts to which I have referred are kept in the University Library in Cambridge:

Mss. Add. 3958 I, 3970, 3975, 3996, 4000. Only small fragments have been published. See, for example, A.R. HALL in The Cambridge Hist. Journal IX, 2 (1948) and also the Royal Society's edition of NEWTON's Correspondence I, p. 103 and *passim*. About NEWTON's first notebook, see GREENSTREET: Isaac Newton 1642—1727, London 1927.

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(A complete account and documentation of HARRIOTT's optical discoveries is expected to appear in Copenhagen in the "Acta Historica Scientarum Naturalium et Medicinalium", presumably before the end of 1962.)

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Zur Lösung des 2. Debeauneschen Problems durch Descartes

Ein Abschnitt aus der Frühgeschichte der inversen Tangentenaufgaben

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Vorgelegt von J. E. HOFMANN

Zusammenfassung *

1638, ein Jahr nach Erscheinen der Descartesschen *Géométrie*, forderte F. DEBEAUNE in einem Aufruf die Lösung einiger Aufgaben des folgenden Typus: Aus einer gewissen Tangenteneigenschaft einer Kurve soll die Kurvengleichung bestimmt werden. Zunächst wird zusammengestellt, was über die Aufgaben aus der erhaltenen Literatur entnommen werden kann, dann folgt in den Abschnitten 4–6 die Behandlung DESCARTES' der uns als einziger vollständig bekannten zweiten Aufgabe, während die Schlußabschnitte der Erläuterung und Würdigung des Descartesschen Vorgehens gewidmet sind. Die Bedeutung der Aufgabe wird ersichtlich aus der Tatsache, daß sich später auch LEIBNIZ, L'HOSPITAL, JAKOB und JOHANN BERNOULLI mit ihr intensiv beschäftigt haben.

1. Einleitung

Die inversen Tangentenaufgaben spielen in der Geschichte der Mathematik deshalb eine besondere Rolle, weil es sich bei ihnen um Quadraturprobleme handelt, die nicht unmittelbar durch eine geometrische Fläche veranschaulicht werden können. Daher waren sie den üblichen Methoden, die zu Beginn des 17. Jahrhunderts in Gebrauch waren, nicht zugänglich. LEIBNIZ z.B. sah gerade in der Tatsache, daß sein Kalkül Aufgaben dieser Art lösen konnte, ein Kriterium für dessen Reichweite, und es tut hier nichts zur Sache, daß er anfangs seine Erfolge in dieser Richtung sehr überschätzt hat.

Der an dieser Stelle zu besprechende Versuch DESCARTES', ein inverses Tangentenproblem Jahrzehnte vorher zu bewältigen, verdient deshalb unser besonderes Interesse. Dies um so mehr, als auch LEIBNIZ die Ausführungen DESCARTES' genau studiert und sich mit ihnen auseinandergesetzt hat. Über ihn kamen dann die übrigen oben genannten Mathematiker zur Beschäftigung mit der 2. Debeauneschen Aufgabe.

Darüber hinaus traten natürlich in andern Zusammenhängen ebenfalls ähnliche Aufgaben auf und wurden z.B. von WALLIS, BARROW und GREGORY diskutiert, so daß wir hier eine der Wurzeln der Theorie der linearen Differentialgleichungen vor uns haben.

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* Der Verfasser ist Herrn J. E. HOFMANN für zahlreiche Hinweise sehr verbunden.

2. Der Aufruf Debeaunes

FLORIMOND DEBEAUNE (1601—1652), Ratsherr am Gericht zu Blois, beschäftigte sich aus Liebhaberei mit Mathematik, Astronomie und Physik. Voller Eifer hatte er sich sofort der Lektüre der Descartesschen *Géométrie* hingegeben und dazu Erklärungen abgefaßt, die später unter dem Titel *Notae breves* von SCHOOTEN in die lateinische Ausgabe mit aufgenommen wurden[1]. Über MERSENNE trat DEBEAUNE mit DESCARTES in Verbindung und ließ ihm Anfang 1639 seine Schrift zugehen, die beim Empfänger auf großes Wohlwollen stieß.

„Es findet sich darin nicht ein einziges Wort, das nicht ganz mit meiner Absicht übereinstimmte,"

schreibt DESCARTES an MERSENNE wie an DEBEAUNE[2]. Diese Wertschätzung DEBEAUNEs von seiten DESCARTES' fand verschiedentlich im Briefwechsel ihren Ausdruck[3], am stärksten wohl, als DEBEAUNE am Jahresende 1640 erkrankt war. Damals schrieb DESCARTES[4]:

«Je prie Dieu pour les âmes de M. Dounot et de Beaugrand. Mais pour Monsieur de Beaune, je prie Dieu qu'il le conserve; car, puisque vous n'avez point de nouvelles de sa mort, je ne la veux pas croire, ni m'en attrister avant le temps; et je le regretterais extrêmement, car je le tiens pour un des meilleurs ésprits qui soient en monde.»

Das Studium der *Géométrie*, worin unter anderem auch das Problem behandelt wird, zu einer gegebenen Kurvengleichung die Tangentengleichung zu finden, ließ in DEBEAUNE die umgekehrte Fragestellung wach werden: Ließe sich wohl eine Methode ersinnen, mittels deren man von einer speziellen Tangenteneigenschaft zurück zur Kurvengleichung gelangen kann? Im Spätsommer 1638 ließ DEBEAUNE eine Aufforderung an die französischen Mathematiker hinausgehen, in der er die Lösung einiger spezieller Aufgaben dieses Typus verlangte. Er mag freilich selbst gesehen haben, daß eine Lösung in voller Allgemeinheit kaum zu erwarten sei. In diesem Sinne drückt sich später auch DESCARTES aus[5]: Er halte es nicht für möglich, eine generelle Umkehrung seiner Tangentenregel oder auch der in mehreren Fällen einfacheren Regel FERMATs anzugeben.

Leider ist DEBEAUNEs Aufruf verschollen. Von besonderer Bedeutung erwies sich die zweite Aufgabe, über die unten ausführlich berichtet wird. Bevor im folgenden Abschnitt das wenige zusammengestellt wird, was wir über die übrigen der vier *Debeauneschen Probleme* aus dem erhaltenen Material entnehmen können, sei erwähnt, daß sich auch DEBEAUNE selbst mit den Aufgaben beschäftigte. Als er DESCARTES das Ergebnis seiner Studien zu einer der Kurven mitteilte, schrieb dieser zurück, er ziehe diesen Debeauneschen Beweis sogar der Archimedischen Quadratur der Parabel vor. Denn, sagt er, ARCHIMEDES habe eine bekannte Kurve untersucht, DEBEAUNE dagegen habe die Fläche unter einer Kurve bestimmt, die selbst nicht gegeben war[5].

3. Die verschollenen Aufgaben

a) Zur dritten Aufgabe liegt nur der Hinweis DESCARTES' vor[5], sie unterscheide sich nur unwesentlich von der zweiten und könne wie diese behandelt werden.

[1] DESCARTES: Geometria (ed. SCHOOTEN), 1649, S. 119/61 = 1659, S. 107/42.
[2] DESCARTES-DEBEAUNE, 20. II. 1639; DESCARTES-MERSENNE, 20. II. 1639.
[3] Zum Beispiel DESCARTES-MERSENNE, 19. VI. 1639.
[4] DESCARTES-MERSENNE, 31. XII. 1640.
[5] DESCARTES-DEBEAUNE, 20. II. 1639.

Daher wäre denkbar, daß es sich um das unter 3d dargestellte Problem handelt. Wahrscheinlicher ist freilich, wie TANNERY bemerkte, die Bedingung einer konstanten Subtangente: $t=y: dy/dx=b$.

b) Über die vierte Aufgabe sind uns nur je eine Bemerkung von DESCARTES und DEBEAUNE erhalten[6], worin beide übereinstimmend feststellten, daß ihre Untersuchung mehr Zeit in Anspruch nähme, als ihnen zur Verfügung stehe. Sie scheint damals nicht gelöst worden zu sein.

c) Etwas mehr wissen wir über die erste Debeaunesche Kurve, die allerdings von anderer Art war. Es handelte sich um eine direkte Tangentenaufgabe. Offenbar war DEBEAUNE damals während seines Studiums der *Géométrie* mit der Descartesschen Tangentenregel noch nicht zurechtgekommen und hatte deshalb dieses Problem mit aufgenommen. Formuliert findet es sich nirgends in dem erhaltenen Briefwechsel, wohl aber in einem Manuskript der *Bibliothèque Nationale*[7], das P. TANNERY in den *Œuvres de Descartes* abgedruckt hat.

Dort wird die erste Kurve beschrieben durch die Proportion $(x+b):x=x:y$, wo b eine Konstante, x und y die beiden Veränderlichen sind. Es folgt als Kurvengleichung $x^2-xy-by=0$. Dies ist (worauf schon TANNERY hinwies) genau das Beispiel, das DEBEAUNE in seinen *Notae breves*[8] verwendet, um die Descartessche Tangentenregel zu erklären. Daß es sich bei der ersten Kurve um eine Hyperbel gehandelt hat, wissen wir auch aus dem Briefwechsel. Schließlich wäre noch zu erwähnen, daß DEBEAUNE damals an ROBERVAL herangetreten ist mit der Bitte, ihm FERMATs Tangentenmethode zukommen zu lassen[9] und als Beispiel die Anwendung auf seine erste Kurve beizufügen. All dies läßt es sicher erscheinen, daß die erste Aufgabe kein inverses, sondern ein direktes Tangentenproblem war.

Dennoch sind die diesbezüglichen Briefstellen von gewissem Interesse. Schon Ende September 1638 hatte DEBEAUNE auf seinen Aufruf hin eine Zuschrift BEAUGRANDs[10] und zwei Wochen später eine solche ROBERVALs[9] erhalten. Inzwischen war er auch selbst mit der ersten Kurve zu Rande gekommen (alle diese drei Untersuchungen sind verschollen) — keiner der Partner hatte jedoch bemerkt, daß eine Hyperbel vorlag. Im Gegenteil: DEBEAUNE hatte wohl erkannt, daß seine Kurve vom 1. *genre* sei laut DESCARTES' Klassifizierung (d.h. ersten oder zweiten Grades in x und y), war aber fest davon überzeugt, es handle sich nicht um einen Kegelschnitt, und glaubte daher, das Descartessche System sei unvollständig. Von DESCARTES (in einem nicht erhaltenen Brief) auf sein Mißverständnis hingewiesen[11], hat dann auch DEBEAUNE eingesehen, daß eine Hyperbel vorliegt[12]. Der tieferblickende DESCARTES freilich konnte sagen[11]: Dies zu erkennen ist so einfach und so klar, daß es nicht einmal notwendig ist, dazu die Feder in die Hand zu nehmen.

Die erwähnten Vorgänge zeigen zunächst an, mit welchen Schwierigkeiten die über MERSENNE miteinander in Verbindung stehenden französischen Mathe-

[6] DESCARTES-DEBEAUNE, 20. II. 1639; DEBEAUNE-MERSENNE, 26. III. 1639.
[7] DO 5, S. 514.
[8] DESCARTES, Geometria (ed. SCHOOTEN), 1649, S. 147/50 = 1659, S. 130/33.
[9] DEBEAUNE-ROBERVAL, 10. X. 1638.
[10] DEBEAUNE-MERSENNE, 25. IX. 1638.
[11] DESCARTES-MERSENNE, 15. XI. 1638.
[12] DEBEAUNE-MERSENNE, 13. XI. 1638.

matiker damals noch zu kämpfen hatten bei ihrem Bemühen, die Descartessche *Géométrie* zu verstehen. Darüber hinaus lassen aber die Briefe DEBEAUNEs und DESCARTES' auch die menschlichen Züge der Korrespondenten MERSENNEs aufleuchten: Die Wichtigtuerei BEAUGRANDs und vor allem ROBERVALs, die zu jedem neuen Problem sofort eine Lösung zu besitzen behaupten — ohne sie freilich preiszugeben —, und die, von DESCARTES durchschaut, ihm dennoch auf die Nerven fallen; die Anerkennung, ja das Lob DESCARTES', wo er, wie bei DEBEAUNE, ernsthaftes Bemühen sieht; die Verehrung, die DEBEAUNE DESCARTES entgegenbringt, und sein Bestreben, die zwischen DESCARTES und ROBERVAL bestehende Animosität nicht noch zu schüren, indem er etwa MERSENNE anweist, den Beitrag ROBERVALs nicht an DESCARTES weiterzugeben, da er ihn für unzureichend hält.

d) Anfang März 1639, kurz bevor er von DESCARTES die ausführliche Untersuchung seiner zweiten Aufgabe erhielt, schrieb DEBEAUNE nochmals an MERSENNE[13] und setzte auseinander, worum es ihm bei seinen Problemen gehe:

Wenn eine Kurve AB mit Scheitel A auf der Achse AD gegeben ist und DB die Ordinate zum Berührungspunkt B der Tangente BC darstellt, dann gibt es ein wohlbekanntes Verfahren, um die Subtangente $CD = t$ zu finden, vorausgesetzt, daß die Beziehung zwischen $AD = x$ und $DB = y$ bekannt ist (Fig. 1). Er verlange umgekehrt eine Methode, schreibt DEBEAUNE, die es ermögliche x zu finden, vorausgesetzt, daß die Beziehung zwischen y und t bekannt sei.

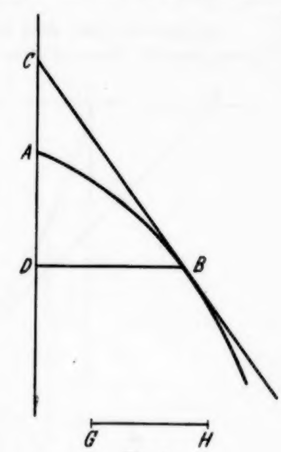

Fig. 1

Und als Beispiel nennt er die Gleichung $t = \beta y/(\beta - y)$ (β konstant), woraus x zu bestimmen sei. Dies kommt wegen $dy/dx = y/t$ auf die Differentialgleichung $y' = 1 - y/\beta$ hinaus. Wenn, so fährt DEBEAUNE fort, BEAUGRAND oder ROBERVAL ihm eine Methode entwickeln könnten, die diese und ähnliche Probleme löse, so wäre er ihnen sehr verpflichtet, und seine zweite Linie ließe sich dann leicht bewältigen, da sie ein Problem dieser Art sei. — Freilich hatte er kurz zuvor noch geäußert:

„Ich erwarte sehnsüchtig die Auffindung meiner Linien, was ich DESCARTES mehr als jedem anderen zutraue, und ich sehe seiner Antwort (die übrigens zu diesem Zeitpunkt bereits unterwegs, d.h. in MERSENNEs Händen war) mit Ungeduld entgegen."

4. Die Formulierung des 2. Problems

DEBEAUNEs zweites Problem, das weitaus bedeutsamste in diesem Zyklus, ist verschiedentlich formuliert worden, so daß der Verlust des ursprünglichen Aufrufs hier nicht schwer wiegt. Von DEBEAUNE selbst haben wir eine Abhandlung vom Oktober 1638[14] für ROBERVAL, in der die Aufgabenstellung wiederholt wird. Es heißt da:

Die Kurve AXE mit Scheitel A und Achse AYZ sei wie folgt bestimmt: Von dem willkürlichen Punkt X der Kurve fälle man die Senkrechte XY auf die Achse AYZ

[13] DEBEAUNE-MERSENNE, 5. III. 1639.
[14] Beilage zu DEBEAUNE-ROBERVAL, 10. X. 1638.

(Fig. 2), zeichne im selben Punkt die Tangente GXN und die Normale XZ (mit den Achsenschnittpunkten G bzw. Z) und gebe noch eine feste Strecke AB vor. Dann bestehe immer das Verhältnis $ZY:YX = AB:(YX - AY)$.

Mit den von DEBEAUNE eingeführten Abkürzungen $AY = y$, $YX = x$, $AZ = v$, $AB = \beta$ ergibt das $(v-y)/x = \beta/(x-y)$. Nun folgt aus der Ähnlichkeit der Dreiecke GYX und XYZ mit dem charakteristischen Dreieck (modern ausgedrückt) die Beziehung $dy/dx = x/(v-y) = (x-y)/\beta$ oder

$$y' = \frac{x-y}{\beta}. \tag{4.1}$$

DEBEAUNE hat den eigenen Lösungsversuch, den er hier anschloß, bald darauf verworfen[15]. Es war ihm jedoch gelungen, einen Ausdruck für die Fläche unter einer seiner Kurven herzuleiten — TANNERY vermutete bereits, es handle sich um die dritte in der Form $y\,dx = b\,dy$ —, wofür ihm DESCARTES, wie schon erwähnt[16], höchstes Lob zollte.

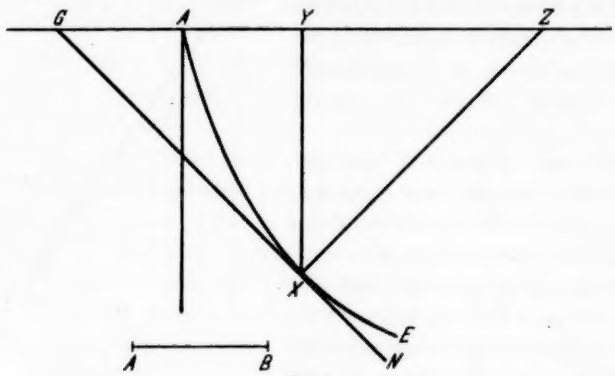

Fig. 2

Einige Jahre später wiederholt DESCARTES die Aufgabe in einem Brief an HAESTRECHT[17]; seine Beschreibung führt auf genau die nämliche Differentialgleichung, die wir eben aus der Erklärung DEBEAUNEs hergeleitet haben. Sie ist also nicht ganz mit der in Abschnitt 3d gegebenen Aufgabe identisch.

ROBERVAL und BEAUGRAND scheinen sich vergeblich an dieser Aufgabe versucht zu haben, ohne freilich ihre Erfolglosigkeit einzugestehen. Jedenfalls spottete DESCARTES, weil sie behaupteten, im Besitz einer Lösung zu sein, dabei aber nicht einmal erkannt hatten, daß die erste Kurve eine Hyperbel war[18]. Immerhin hatte ROBERVAL die Existenz einer Asymptote nachgewiesen[19]. Ebenso scheint, dem Urteil DESCARTES' zufolge, FERMATs Lösung unzulänglich gewesen zu sein[20].

DESCARTES' erste Antwort an DEBEAUNE vom 11. Okt. 1638[21] ist verschollen; wir wissen davon nur aus einem späteren Brief[20]. Jedoch besitzen wir seine ausgedehnte Untersuchung der zweiten Kurve vom 20. Febr. 1639[22], der wir uns nun zuwenden wollen.

[15] DEBEAUNE-MERSENNE, 13. XI. 1638.
[16] S. 407 unten.
[17] DESCARTES-HAESTRECHT, VI (?) 1645.
[18] DESCARTES-MERSENNE, 15. XI. 1638.
[19] DEBEAUNE-ROBERVAL, 10. X. 1638.
[20] DESCARTES-MERSENNE, 9. II. 1639.
[21] Beilage zu DESCARTES-MERSENNE, 11. X. 1638.
[22] DESCARTES-DEBEAUNE, 20. II. 1639.

5. Descartes' Konstruktion der Lösungsfunktion

DESCARTES' Grundgedanke ist, einen allgemeinen Punkt der unbekannten Kurve als Grenzlage des Schnittpunktes zweier benachbarter Tagenten zu bestimmen. Man kann hierin bereits eine Vorahnung des Richtungsfeldes einer linearen Differentialgleichung sehen. Fig. 3 gibt bis auf geringfügige Ergänzungen die Zeichnung DESCARTES' wieder; AVX ist die gesuchte Kurve mit Scheitel in A (d.h. vertikaler Tangente). Als Bezugssystem dienen die horizontale Gerade BAY (Y-Achse) und die dazu senkrechte Gerade AC (X-Achse).

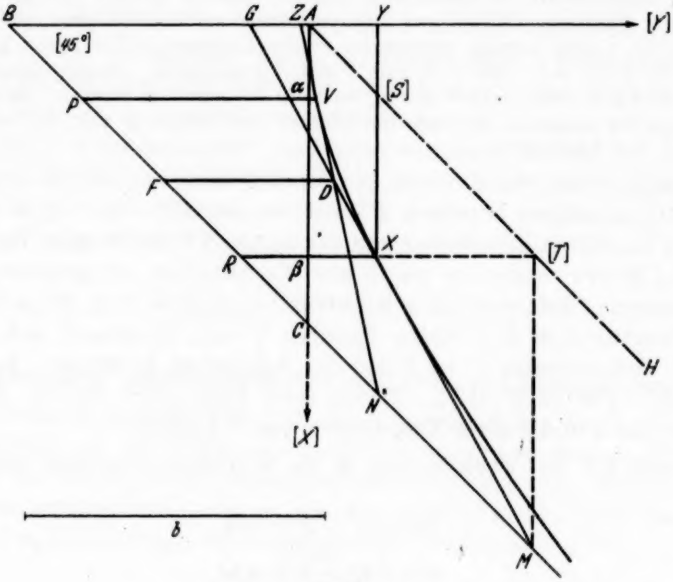

Fig. 3

Die Wahl der Bezeichnungen stimmt mit derjenigen DEBEAUNEs im vorigen Abschnitt überein, nur verwendet DESCARTES für die Konstante b anstelle von β. Es sei aber besonders hervorgehoben, daß DESCARTES x als Funktion von y betrachtet, d.h. die Rollen der Veränderlichen gegenüber dem heute üblichen Gebrauch vertauscht. Wir hätten daher auch die Differentialgleichung in der Form

$$\frac{dx}{dy} = \frac{b}{x-y} \tag{5.1}$$

zu schreiben.

Die um 45° geneigte Gerade BC wird ohne Beweis als Asymptote der gesuchten Kurve bezeichnet. Ebenfalls ohne Beweis wird mitgeteilt, daß die durch die Tangenten AC, ZVN und GXM in den Punkten A, V und X bestimmten Strecken BC, PN und RM alle gleich lang sind.

Diese Eigenschaften lassen sich folgendermaßen zeigen. Durch die Tangente im allgemeinen Kurvenpunkt X erhält man zwei ähnliche Dreiecke GYX und XTM, daraus

$$\frac{XY}{YG} = \frac{MT}{TX}. \tag{5.2}$$

Andererseits kann die Ausgangsgleichung

$$\frac{XY}{YG} = \frac{AB}{XY - AY} \tag{5.3}$$

fortgesetzt werden:

$$\frac{XY}{YG} = \frac{AB}{XY - AY} = \frac{AB}{XS} = \frac{AB}{TX} = \frac{b}{TX}. \tag{5.4}$$

Daher ist $MT = b$, also $RM = PN = BC = b\sqrt{2}$, wie behauptet.

Hieraus folgt weiter, daß BC Asymptote ist; denn man kann sich jetzt die Kurve AVX durch eine Parallelverschiebung des Dreiecks RTM im durch BC und AH begrenzten Diagonalstreifen entstanden denken, wobei X auf RT beweglich ist. Geht man von der Position $RXTM$ aus und verschiebt nach links oben, dann muß der Punkt X nach rechts bewegt werden, da er die Tangente GXM nicht überschreiten kann. In der Lage BAC fallen X und T mit A zusammen. Bewegt man umgekehrt das Dreieck RTM nach rechts unten, so muß X gegen R streben; dabei geht das Verhältnis b/TX gegen 1, so daß der Winkel der Tangente mit BA 45° zustrebt. Folglich ist BA Asymptote.

DESCARTES denkt sich die feste Strecke $AB = b$ in m gleiche Teile geteilt und die zu AB parallelen Strecken $PV = nb/m$ und $RX = (n-1)b/m$ $(0 < n < m$; n, m ganz) in solcher Entfernung von der Achse BY abgetragen, daß V und X Punkte der Kurve werden; er macht also die Annahme, die gesuchte Kurve sei bereits gegeben. Alsdann ist diese Konstruktion *möglich*, da ja BC Asymptote ist.

Die Tangenten in den beiden Punkten V und X müssen sich schneiden; durch den Schnittpunkt D ist F auf der Asymptote bestimmt. Jetzt ist, wie gesagt, $BC = PN = RM = b\sqrt{2}$. Ferner setzt DESCARTES $PF = \varepsilon$, $FR = \omega$ und bezeichnet mit α und β die x-Koordinaten von V und X.

Nun wird FD auf zweierlei Art in ein Verhältnis eingebaut und dann eliminiert:

$$FD:FN = PV:PN, \tag{5.5}$$

$$FD:FM = RX:RM, \tag{5.6}$$

$$FD = PV \cdot FN/PN = RX \cdot FM/RM, \tag{5.7}$$

und wegen $PN = RM$

$$PV \cdot (PN - PF) = RX \cdot (FR + RM). \tag{5.8}$$

Unter Benutzung der Abkürzungen folgt daraus nach Multiplikation mit m und Division durch b:

$$n(b\sqrt{2} - \varepsilon) = (n-1)(\omega + b\sqrt{2}). \tag{5.9}$$

Hieraus läßt sich $PR = PF + FR = \varepsilon + \omega$ auf doppelte Art darstellen:

$$\varepsilon + \omega = (b\sqrt{2} - \varepsilon)/(n-1) = (b\sqrt{2} + \omega)/n, \tag{5.10}$$

so daß PR zwischen den Schranken

$$b\sqrt{2}\,n \quad \text{und} \quad b\sqrt{2}/(n-1)$$

eingeschlossen ist. Das bedeutet für die Differenz der x-Koordinaten der Punkte X und V:

$$\frac{b}{n} < \beta - \alpha < \frac{b}{n-1}. \tag{5.11}$$

Diese Überlegung läßt sich für jedes beliebige Paar von Kurvenpunkten durchführen, z.B. auch für A und V. Indem DESCARTES mit diesen beiden Punkten beginnt, aber zunächst hier noch einmal einen Zwischenpunkt einschiebt, erhält er als Beispiel mit $m=8$, $n=6$

$$\left(\tfrac{1}{8}+\tfrac{1}{7}\right)b<\alpha<\left(\tfrac{1}{7}+\tfrac{1}{6}\right)b, \tag{5.12}$$

dann durch Verfeinerung der Unterteilung (mit $m=16$)

$$\left(\tfrac{1}{16}+\tfrac{1}{15}+\tfrac{1}{14}+\tfrac{1}{13}\right)b<\alpha<\left(\tfrac{1}{15}+\tfrac{1}{14}+\tfrac{1}{13}+\tfrac{1}{12}\right)b. \tag{5.13}$$

Indem man auf diese Weise die Strecke $AB=b$ in mehr und mehr Teile unterteilt, kann man sich der genauen Länge der x-Koordinaten immer mehr bis ins Unendliche annähern und so die Kurve mechanisch konstruieren, sagt DESCARTES.

Das Verfahren läuft demnach darauf hinaus, den Abstand des gesuchten Kurvenpunktes von der Asymptote (und dadurch seine y-Koordinate) als Bruchteil der festen Strecke AB vorzugeben und daraus mittels der beschriebenen Methode die zugehörige x-Koordinate beliebig genau zu bestimmen. Ist dieser Abstand gleich nb/m, so wird x eingeschränkt durch die Grenzen

$$\left(\frac{1}{m}+\frac{1}{m-1}+\cdots+\frac{1}{n+1}\right)b<x<\left(\frac{1}{m-1}+\frac{1}{m-2}+\cdots+\frac{1}{n}\right)b. \tag{5.14}$$

6. Descartes' mechanische Beschreibung der Lösungskurve

Obwohl damit die Aufgabe gelöst ist, soweit das bei dieser Art des Vorgehens überhaupt möglich ist, bleibt DESCARTES hierbei noch nicht stehen. Betrachtet man nämlich bei vorgegebener Anzahl m von Teilpunkten der Strecke b zwei Kurvenpunkte im Abstand x_1 und x_2 von der Achse AY, bestimmt durch zwei Zahlen n_1 und n_2 $(0<n_1<n_2\leqq m)$, so gilt für ihre Differenz

$$\left(\frac{1}{n_2}+\frac{1}{n_2-1}+\cdots+\frac{1}{n_1+1}\right)b<x_1-x_2<\left(\frac{1}{n_2-1}+\frac{1}{n_2-2}+\cdots+\frac{1}{n_1}\right)b. \tag{6.1}$$

Dies folgt am raschesten aus (5.14), angewandt auf x_1 und x_2; denn dann darf man entsprechende Seiten der Ungleichungen voneinander subtrahieren, was hier ausnahmsweise zulässig ist, weil die Summen auf beiden Seiten durch gliedweise Addition aus der grundlegenden Beziehung (5.11) gewonnen wurden. Jetzt wird jedes Glied auf der linken Seite durch das kleinste, auf der rechten durch das größte ersetzt. Das ergibt

$$\frac{n_2-n_1}{n_2}\,b<x_1-x_2<\frac{n_2-n_1}{n_1}\,b \tag{6.2}$$

oder

$$\frac{m}{n_2}\left(\frac{n_2}{m}-\frac{n_1}{m}\right)b<x_1-x_2<\frac{m}{n_1}\left(\frac{n_2}{m}-\frac{n_1}{m}\right)b. \tag{6.3}$$

Bei DESCARTES ist diese allgemeingültige Relation, die sich in den Anmerkungen TANNERYs findet, nur wortreich für den Fall $m=8$ beschrieben. — Um die daran anschließende mechanische Erklärung der Kurve besser darstellen zu können, fasse ich DESCARTES' spezielle Ergebnisse in der folgenden Tabelle zusammen.

Mit $m=8$ erhalten wir die x-Koordinaten x_i $(i=1, 2, \ldots, 8)$ von acht Punkten aus $n_i=1, 2, \ldots, 8$ und betrachten paarweise aufeinanderfolgende beginnend

beim Scheitelpunkt A, d.h. mit $n=8$ (Fig. 3). Dann ist also immer $n_{k+1}-n_k=1$, und wir erhalten

$$\begin{aligned}
\tfrac{8}{8}\cdot\tfrac{1}{8}b &< x_7 - x_8 < \tfrac{8}{7}\cdot\tfrac{1}{8}b\\
\tfrac{8}{7}\cdot\tfrac{1}{8}b &< x_6 - x_7 < \tfrac{8}{6}\cdot\tfrac{1}{8}b\\
\tfrac{8}{6}\cdot\tfrac{1}{8}b &< x_5 - x_6 < \tfrac{8}{5}\cdot\tfrac{1}{8}b\\
&\cdots\cdots\cdots\\
\tfrac{8}{2}\cdot\tfrac{1}{8}b &< x_1 - x_2 < \tfrac{8}{1}\cdot\tfrac{1}{8}b.
\end{aligned}\tag{6.4}$$

Die Punkte mit den x-Koordinaten $x_8, x_7, \ldots, x_1$ haben der Reihe nach die Abstände $\frac{8}{8}b, \frac{7}{8}b, \ldots, \frac{1}{8}b$ von der Asymptote BC, gemessen parallel zur Richtung der Achse AY, während ihre gegenseitigen Abstände in der x-Richtung durch diese Ungleichungen bestimmt sind. Will man noch weitere Punkte in größerer Entfernung vom Scheitel A betrachten, so muß man mit DESCARTES zu größeren Werten von m übergehen. Zur Unterscheidung seien die zu $m=16$ gehörigen x-Koordinaten durch $\bar{x}_i$ bezeichnet, die den Wert $m=32$ entsprechenden mit $\bar{\bar{x}}_i$. Dann ist $\bar{x}_2=x_1$, $\bar{\bar{x}}_2=\bar{x}_1$, und „weiter draußen" liegen nur noch die Punkte $\bar{x}_1$ und $\bar{\bar{x}}_1$. Für sie lauten die Bedingungen

$$\begin{aligned}
\tfrac{16}{2}\cdot\tfrac{1}{16}b &< \bar{x}_1 - \bar{x}_2 < \tfrac{16}{1}\cdot\tfrac{1}{16}b\\
\tfrac{32}{2}\cdot\tfrac{1}{32}b &< \bar{\bar{x}}_1 - \bar{\bar{x}}_2 < \tfrac{32}{1}\cdot\tfrac{1}{32}b.
\end{aligned}\tag{6.5}$$

Alle hier angegebenen Werte stehen im Brief DESCARTES'. Auf sie bezieht sich die mechanische Erzeugung der Kurve:

Verschiebt man die zu BC parallele Gerade AH mit konstanter Geschwindigkeit parallel zu sich selbst nach links, dann passiert sie in festen Zeitabständen die Punkte auf BA im Abstand $7b/8, 6b/8, \ldots, b/8$ von B und damit auch die durch $n_i=7, 6, \ldots, 1$ bestimmten Punkte der Kurve. Beginnend im selben Augenblick und mit derselben Geschwindigkeit, bewege sich eine zweite Gerade von der Ausgangslage BA parallel zu sich selbst nach unten, doch nehme ihre Geschwindigkeit derart zu, daß sie $\frac{8}{7}$ der Ausgangsgeschwindigkeit erreicht hat, wenn die erste Linie $b/8$ zurückgelegt hat; $\frac{8}{6}$, wenn die erste Linie $2b/8$ zurückgelegt hat usw. Da aber der Abstand der zweiten mit zunehmender Geschwindigkeit bewegten Geraden von der Achse AY gleich x ist, stellt die Bewegungsvorschrift nichts anderes dar als die Kette der genannten Ungleichungen für $m=8$. M.a.W., der Schnittpunkt der beiden beweglichen Geraden beschreibt die gesuchte Kurve. — Will man vom letzten der so bestimmten Punkte zu einem weiteren fortschreiten, dann muß man kleinere Zeitabschnitte ins Auge fassen; DESCARTES halbiert die gewählte Zeiteinheit zweimal und erhält so zwei weitere Punkte der Kurve. Dies läßt sich beliebig fortsetzen, so daß auf diese Weise alle Kurvenpunkte, die einen rationalen Abstand von der Asymptote BC haben (gemessen parallel zur Achse AY), gefunden werden können.

Freilich ist DESCARTES von diesem Ergebnis nicht entzückt. Die beiden Bewegungen seien, so führt er aus, inkommensurabel in dem Sinn, daß die eine nicht exakt durch die andere bestimmt sei (sondern nur mittels der Ungleichungen). So gehöre die Kurve zu denen, die er in der *Géométrie* verworfen habe, weil sie nur mechanisch (nicht aber algebraisch) definiert seien. Daher wundere es ihn auch nicht, daß er mit ihr nicht auf anderem Weg zum Ziel gekommen sei[23].

[23] Vgl. für die Beziehungen zu DESCARTES' Metaphysik die Studien von BELAVAL und VUILLEMIN.

7. Interpretation des Descartesschen Verfahrens

Hiermit enden DESCARTES' Ausführungen zu diesem Problem. Die weitere Diskussion sei mit einer knappen Interpretation in moderner Schreibweise eingeleitet.

Die Ungleichung (5.14) kann, ähnlich wie das für (6.1) ausgeführt wurde, in die Form

$$\frac{m-n}{m}\,b < x < \frac{m-n}{n}\,b \tag{7.1}$$

oder

$$\frac{1}{m} < \frac{x/b}{m-n} < \frac{1}{n} \tag{7.2}$$

gebracht werden. Durch Vergleich mit der für den Logarithmus geltenden Beziehung

$$\frac{1}{m} < \frac{\ln m - \ln n}{m-n} < \frac{1}{n}. \tag{7.3}$$

folgt daraus

$$\frac{x}{b} = \ln m - \ln n = -\ln\frac{n}{m}. \tag{7.4}$$

Setzen wir noch

$$z = \frac{n\,b}{m} \tag{7.5}$$

für den horizontalen Abstand von der Asymptote BC, dann haben wir schließlich

$$x = -\,b\ln\frac{z}{b}. \tag{7.6}$$

Diese logarithmische Funktion ist auf das schiefwinklige Koordinatensystem bezogen, das von der Y-Achse zusammen mit der Asymptote gebildet wird. Es stellt ja das eigentliche Bezugssystem DESCARTES' dar, wenn das auch nirgends ausgesprochen wird.

Im X, Y-System lautet die Gleichung der Asymptote

$$y = x - b; \tag{7.7}$$

der allgemeine Kurvenpunkt ist bestimmt durch

$$y = z + x - b. \tag{7.8}$$

Dann ist

$$dy = dz + dx. \tag{7.9}$$

Einsetzen in die Differentialgleichung (5.1)

$$\frac{dx}{dy} = \frac{b}{x-y}$$

ergibt

$$\frac{dx}{dz+dx} = \frac{b}{b-z}. \tag{7.10}$$

daraus folgt

$$\frac{dx}{dz} = -\frac{b}{z}, \tag{7.11}$$

also

$$\frac{x}{\ } = -\ln\frac{z}{b} = \ln\frac{b}{z}. \tag{7.12}$$

Beim mechanischen Verfahren von DESCARTES wäre

$$y = x - b\,t \tag{7.13}$$

(t = Parameter der Zeit, $0 \leqq t \leqq 1$) die Gleichung der mit konstanter Geschwindigkeit von der Ausgangslage AH aus nach links laufenden Geraden. Das ergibt nach t aufgelöst

$$t = \frac{x-y}{b} = \frac{b-z}{b} = 1 - \frac{z}{b}. \tag{7.14}$$

Die Geschwindigkeit der nach unten laufenden Geraden ist durch

$$\frac{dx}{dt} = \frac{b}{1-t} \tag{7.15}$$

gegeben, wie aus den Tabellen (6.4), (6.5) und ihrer Erklärung hervorgeht. Daraus ergibt sich

$$x = -b \ln(1-t), \tag{7.16}$$

wobei die Integrationskonstante wegen der Anfangsbedingung $x=0$ für $t=0$ verschwindet. Elimination von t liefert in Übereinstimmung mit (7.6)

$$x = -b \ln \frac{z}{b}.$$

8. Schlußbetrachtung

Es stellt sich nun die Frage: Wieso weist DESCARTES mit keinem Wort, nicht einmal mit irgendeiner Andeutung, auf die Verwandtschaft der Lösungskurve mit dem Logarithmus hin? Ist es möglich, daß er diese Beziehung gar nicht gesehen hat? Oder was könnte ihn davon abgehalten haben, die logarithmische Funktion zu erwähnen?

Die Ausführungen von DESCARTES enthalten, wie wir gesehen haben, an zwei Stellen Beziehungen, die für uns heute sofort die Verbindung zum Logarithmus herstellen: In Abschnitt 5 wird die Herleitung der Stammbruchnäherungen beschrieben, in Abschnitt 6 begegnen wir in den Ungleichungen (6.4) und (6.5) der Zuordnung einer geometrischen zu einer arithmetischen Folge; denn die drei letzten Relationen dort besagen ja, daß der festen Spanne $\frac{1}{2}$ für x die Abstände $\frac{1}{8}$, $\frac{1}{16}$, $\frac{1}{32}$ von der Asymptote entsprechen.

Zu den Stammbruchnäherungen wäre zu sagen, daß sie — abgesehen von unserem Beispiel — zuerst bei P. MENGOLI in der *Geometria speciosa* (1659) auftreten, wo sie aus der Proportionalität des Logarithmus zur Hyperbelfläche abgeleitet sind. Diese Beziehung findet sich erstmalig in dem 1622—25 entstandenen, jedoch erst 1647 zum Druck gekommenen *Opus geometricum* des GREGORIUS A S. VINCENTIO und in einer Abhandlung seines Schülers SARASA aus dem Jahr 1649[24]. Es ist sicher, daß sie DESCARTES 1639 noch nicht bekannt war. Seine Ableitung der Näherungen ist ja auch viel komplizierter als die Abschätzung für

$$\ln \frac{m}{n} = \int_n^m \frac{dt}{t} \tag{8.1}$$

durch Treppenfiguren.

Die Einführung der Logarithmen auf Grund der Zuordnung einer arithmetischen zu einer geometrischen Folge ist dagegen zu dieser Zeit schon sehr ver-

[24] HOFMANN I, S. 149/50.

breitet gewesen. Als Beleg zitiere ich nur aus NAPIERs *Constructio* (1619/20) Satz 26:

„Der Logarithmus eines gegebenen Sinus [gemeint ist: Strecke oder Zahl] ist diejenige Zahl, die arithmetisch mit derselben konstanten Geschwindigkeit angewachsen ist, mit der der Radius begann, auf geometrische Weise abzunehmen ..."

Die Übereinstimmung mit der entsprechenden Zuordnung DESCARTES' geht also so weit, daß bei beiden Autoren einer wachsenden arithmetischen Folge eine abnehmende geometrische Folge zugeordnet wird — abweichend vom heutigen Gebrauch, wo beide Folgen monoton zunehmen. Übrigens sind die Napierschen Logarithmen nicht mit den natürlichen identisch. Will man ihnen eine Basis im heutigen Sinn zuweisen, so hat man eine Maßstabsänderung vorzunehmen und erhält angenähert

$$\frac{1}{e}\left(1-\frac{1}{3}\cdot 10^{-14}\right),$$

also ungefähr $1/e$ dafür[25].

Ob nun DESCARTES an dieser Stelle einfach nicht an das Napiersche Werk gedacht hat, ist schwer zu sagen. Da er die Werke seiner Zeitgenossen oft nur flüchtig studierte und lieber alles allein entdeckt haben wollte, mag ihm die Parallelstelle bei NAPIER nicht gegenwärtig gewesen sein, doch ist ebensogut möglich, daß er es vorzog, die Kenntnis dieses Tatbestandes zu verschweigen. Sollte das der Fall sein, dann währe *ihm* die Entdeckung der natürlichen Logarithmen und der oben genannten Mengolischen Näherungen zuzuschreiben, das erstere allerdings mit einer gewissen Einschränkung, die sich aus dem Folgenden ergibt.

DESCARTES' Verfahren ist trotz der in Abschnitt 5 erwähnten Bemerkung, es lasse sich unendlich fortsetzen, im Grunde finitesimal. DESCARTES beginnt zwar seine Ausführungen mit der Grenzlage zweier benachbarter Tangenten, doch wird der eigentliche Grenzübergang nicht vollzogen; er übersteigt die Möglichkeiten von DESCARTES. So endet die Konstruktion mit einer Approximation und deren Übertragung in ein mechanisches Verfahren, das nicht auf algebraische Weise beschrieben werden kann. Daher gehört die Kurve auch nicht zu denen, die DESCARTES in seiner *Géométrie* behandelt, vielmehr zu jenen, die er ausgeschlossen hat. Sie ist ein Beispiel für die Approximationsmathematik, nicht aber für die Präzisionsmathematik, die allein für ihn die wahre Mathematik ausmacht. Mag sein, daß diese Erkenntnis für DESCARTES so ernüchternd wirkte, daß er kein weiteres Wort mehr über die Kurve verlieren wollte; denn mit dieser Feststellung endet er ja, wie wir sahen, den Brief an DEBEAUNE. Wie dem auch sei: Selbst wenn DESCARTES dabei an die Napierschen Logarithmen gedacht haben sollte, hatte er wegen des fehlenden Schritts zur Infinitesimalmathematik den natürlichen Logarithmus noch nicht in dem Sinn verstanden, wie wir ihn ansehen, nämlich als Ergebnis eines Grenzwertprozesses.

Und schließlich ist noch ein weiterer Punkt zu berücksichtigen. Als die Logarithmen im frühen 17. Jahrhundert eingeführt wurden, dachte noch niemand an eine graphische Darstellung dieser „Funktion" im heutigen Sinne; sie wurden immer als Zuordnung einer arithmetischen zu einer geometrischen Reihe auf-

[25] TROPFKE II (3. Aufl. 1933), S. 216/18.

gefaßt[26]. Der Funktionsbegriff entsteht ja erst bei LEIBNIZ[27], und die Auffassung des Logarithmierens als die zweite Umkehrung des Potenzierens erscheint erst bei EULER[26]. Daher war es durchaus nicht so naheliegend, beim Anblick eines Kurvenbildes an den Logarithmus zu denken, wie uns das heutzutage erscheint. Die erste bekannte Darstellung dieser Art ist in einer damals ungedruckt gebliebenen Studie TORRICELLIs aus dem Jahr 1647 enthalten; ähnliche Bemerkungen treten erst 1671 in den *Elémens de géométrie* von PARDIES auf[28].

Abschließend bleibt also festzustellen, daß lediglich die Zuordnung einer arithmetischen zu einer geometrischen Folge Anlaß für DESCARTES geboten haben konnte, das Vorliegen eines Logarithmus zu erkennen. Sein Ansatz führt auf den natürlichen Logarithmus und, wenn auch auf umständliche Weise, auf die Stammbruchnäherungen. Die entscheidende Lücke in DESCARTES' Ausführungen ist durch die Begrenzung seines mathematischen Systems begründet: Für die Grenzbetrachtung, den Schritt zur Infinitesimalmathematik, ist darin kein Raum. Seine Konstruktion muß, wie BELAVAL sehr schön ausgeführt hat[29], finitesimal verstanden werden.

Das erneute Studium dieser Aufgabe setzt erst wieder fast 40 Jahre später mit LEIBNIZ ein. Sie hat ihn und andere eingangs bereits erwähnte Forscher noch lange beschäftigt.

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Nicole Oresme and the Commensurability or Incommensurability of the Celestial Motions

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The general problem of commensurability and incommensurability has been a central theme in the history of mathematics. In the fourteenth century, NICOLE ORESME was completely captivated by this subject and made it the focal point of the strictly mathematical chapters dealing with proportionality in his *De proportionibus proportionum.*[1] His overall treatment may have been unique and appears to have influenced some later authors.[2] But it appears that he was even more taken with the application of the concept of commensurability and incommensurability to bodies, or mobiles, in motion on circumferences of circles and particularly with the best concrete exemplification of such motion, namely the movements of celestial bodies. ORESME'S most extensive and concentrated exposition of this is found in an unpublished work whose very title reveals this major interest—*De commensurabilitate vel incommensurabilitate motuum celi*[3] *(On the Commensurability or Incommensurability of the Motions of the Heavens).* The purpose of this article is to elucidate the most important parts of this treatise.

Following upon a general introduction, the *De commensurabilitate* is divided into three separate parts. Part I contains twenty-five propositions, or conclusions, all of which assume that the bodies, or mobiles, in circular motion have velocities which are mutually commensurable. In Part II all the velocities in the twelve propositions, with a minor exception, are mutually incommensurable. There are no further propositions in Part III and instead we find a series of arguments on

[1] Possibly written *ca.* 1360. For a summary account see EDWARD GRANT, Nicole Oresme and his De proportionibus proportionum. Isis **51**, 293—314 (1960).

[2] Two authors influenced by ORESME'S treatment of the incommensurability of proportions were ALVARUS THOMAS in his Liber de triplici motu (Paris, 1509) and GEORGE LOKERT in his Tractatus proportionum published in Questiones et decisiones physicales insignium virorum (Paris, 1518).

[3] Though the manuscripts give variant titles, this would seem correct since ORESME cites it exactly this way in his French translation of, and commentary on, ARISTOTELE'S De Caelo. See A.D. MENUT & A. J. DENOMY, Maistre Nicole Oresme: Le Livre du Ciel et du Monde, Text and Commentary, in Mediaeval Studies **3**, 253, 255 (1941).

A briefer treatment of the commensurability and incommensurability of the celestial motions is found in what purports to be chapters five and six of ORESME'S De proportionibus proportionum but may be a separate treatise of which quite a few manuscripts have already been identified. I have referred to it by its incipit as *Ad pauca respicientes.* The connections between the two treatises have yet to be determined and must await a careful comparison of their respective propositions. For a modicum of further information see GRANT, *op. cit.*, p. 296—297, n. 17. Some other of ORESME'S treatises which allude to this subject are mentioned on pp. 311—312, n. 64.

behalf of both Commensurability and Incommensurability to determine the one more appropriate for celestial motion.

In this article, except for a few concepts and definitions cited from the introduction, only Parts I and II will be summarized and discussed. The general procedure, following the introductory material, will be to provide an explicative summary of the successive propositions in each part. Immediately preceding the discussion of each proposition there will be found the appropriate Latin text[4] and translation of the enunciation. Exhaustive citation of supporting Latin passages, however, is not attempted, since this would be tantamount to reproducing the entire Latin text. Only the most significant and interesting ideas and concepts will be quoted.

Introduction

Of the greatest importance is ORESME'S declaration of intent. He explains that he will consider only *exact*—not approximate—punctual aspects of mobiles moving in circular motion. ORESME is perfectly aware that astronomers are not concerned with such unattainable exactitude and are content to avoid sensible and detectable error, although he notes that minute undetectable error when multiplied through some period of time will produce sensible error.[5]

The terms commensurability and incommensurability as applied to circular motion pertain either to parts of circles traversed (angles) or to the number of times whole circles are traversed. Commensurability obtains when, in equal times, mobiles describe mutually commensurable angles around the centers of their circles; or, when in commensurable times each mobile completes an integral number of circulations (see below for definition of the term *circulatio*). Incommensurability will be had when in equal times incommensurable angles are described with respect to the centers of the circles[6]; or, if the times are incommensurable, then the circulations must be incommensurable.[7]

[4] The Latin text has been edited from the following manuscripts of the De commensurabilitate: Vat. lat. 4082, ff. 97v—108v; Cambridge, Peterhouse 277, Bibliotheca Pepysiana 2329, ff. 111v—128r; Bibliothèque Nationale, Arsenal 522, ff. 110r—121r. Three other manuscripts listed by MENUT & DENOMY, *op. cit.*, **5**, 246 (1943) are: Bibliothèque Nationale MS. lat. 7281, ff. 259—273; Florence, Laurentian Library, Ashburnham 210, ff. 159—171v; Utrecht (Rijksuniversiteit) MS. 725, ff. 172—193. It should be noted that Pepys 2329 lacks almost the whole of the introduction and in the *explicit* (fol. 128r) erroneously attributes the work to JORDANUS DE NEMORE. Although this is a composite text, I shall always cite the corresponding passage in Vat. lat. 4082 so the reader may have a specific folio and column reference.

[5] "Intentio in hoc libello est loqui de precisis et punctualibus aspectibus mobilium circulariter, et non de aspectibus prope punctum de quibus communiter intendunt astronomi qui non curant nisi quod non sit sensibilis defectus, quamvis modicus error imperceptibilis multiplicatus per tempus notabilem effectum efficat" (Vat. lat. 4082, 98r, c. 1). Pepys 2329 lacks this part of the introduction.

[6] This applies to separate non-concentric circles, concentric circles, and eccentric circles.

[7] "Commensurabilitatem et incommensurabilitatem motuum circularum accipio penes quantitatem angulorum descriptorum circa centrum, aut celorum sive in respectu circulationum, quod idem est, ita quod ista moventur commensurabiliter cum in temporibus equalibus describunt angulos commensurabiles circa centrum; sive, que in temporibus commensurabilibus suas circulationes perficiunt. Et circulationes sunt incommensurabiles que in temporibus incommensurabilibus fuerint; et quibus describuntur temporibus equalibus anguli incommensurabiles circa centrum" (Vat. lat. 4082, f. 97v, c. 2).

An important distinction is that between use of the terms *circulatio* and *revolutio*.[8] *Circulatio* applies to a single mobile only and it is said to complete one circulation when it has moved from a given point back to that same point. *Revolutio* is used of two or more mobiles which have moved from some definite aspect (conjunction, opposition, *etc.*) back again to that same aspect. If, for example, two mobiles are in conjunction in point *d*, they complete a *revolutio* at the moment they return to conjunction in *d*.

Since in almost all of the conclusions conjunctions of mobiles stand as paradigm cases for all the other aspects, ORESME is obliged to define his use of the term *coniunctio*. In concentric motions a conjunction occurs when the centers of two or more mobiles lie on the same line drawn from the center of their concentric circles.[9] For a physical conjunction of celestial bodies it is required that they be on the same surface or great circle which intersects the poles of the universe.[10] Indeed, they must be simultaneously on the same meridian. In a conjunction of physical bodies it is not necessary that the line drawn from the center of the world intersect the centers of the planets, but only that the planets be on the same meridian.

Part I. Commensurable velocities

Proposition I

Si fuerint quotlibet numeri ab unitate continua proportionalitate dispositi, nullus eorum numeratur ab aliquo primo numero nisi ab illo vel ab illis, si fuerint, qui numerant illum qui in illa proportionalitate immediate sequitur unitatem.

If any whatever numbers are arranged in continuous proportionality beginning with unity no prime number would measure (or number) any of them unless the prime number, or prime numbers, measure the number immediately following unity.

The proof of Proposition I depends on EUCLID IX, 11[11] where it is demonstrated that if a prime number measures the last number in a geometric progression, it must also measure the number immediately following unity. ORESME, however, wishes to show the converse of this, namely that if the prime number measures the number following immediately after unity it will measure the other numbers in the series. To prove this he argues by denying the consequent *(a destructione*

[8] "Circulationem voco unius mobilis circulatio de aliquo puncto ad eandem reditionem; revolutionem, vero, plurium mobilium de aliquo statu reditionem ad statum sive aspectum omnino consimilem" (Vat. lat. 4082, f. 98r, c. 1). A *circulatio* is analogous to a sidereal period in astronomy. A *revolutio* has no real counterpart in modern astronomy since it requires the mobiles to return to conjunction or opposition with reference to some point or points fixed in space. It is, however, quite like the concept of the Great Year in ancient and medieval astrology. (See notes 43 and 44.)

[9] "Voco ergo pro nunc coniunctionem aliquorum mobilium quando eorum centra sunt in eadem linea egrediente a centro" (Vat. lat. 4082, f. 98r, c. 1). This does not apply when the circles are eccentric and probably the phrase "pro nunc" is intended to convey this qualification.

[10] "Tunc est coniunctio corporalis sive saltem in eadem superficie sive circulo transeunte per polos mundi. Sint in eodem meridiano sive per polos orbis signorum ..." (Vat. lat. 4082, f. 98r, c. 1).

[11] This Euclidean proposition is so numbered in the edition of CAMPANUS OF NOVARA's commentary and text of EUCLID's *Elements*. Euclidis Megarensis mathematici clarissimi Elementorum geometricorum libri XV (Basileae, per Iohannem Hervagium, 1546), pp. 114—115. In the modern edition it appears as IX, 12. See Sir THOMAS L. HEATH, The Thirteen Books of Euclid's Elements (New York 1956), II, p. 397.

consequentis), that is, he asserts that if the prime number *does not* measure the number immediately after unity it will not measure the last number in the series, and in the same manner it can be shown that it will not measure any other number in the series.

Proposition II

Si per ymaginationem aliquod continuum dividatur in aliquot partes et quelibet illarum in totidem, et sic in infinitum, in nullo puncto cadet divisio in quo caderet si divideretur secundum aliam proportionalitatem, nisi numeri immediate sequentes unitatem illarum proportionalitatum sint communicantes.

If, by the imagination, some continuum could be divided into aliquot parts and any of these into aliquot parts, and so *ad infinitum*, no point of this division will coincide with any point of another division which has divided the continuum according to another proportionality unless the numbers directly following unity in both proportionalities are commensurable.

Relations between two geometric progressions are considered next. First, however, ORESME establishes that conclusions which are true about straight line continua are also true of circular continua. The only distinction is that whereas one point will divide a straight line, it takes at least two points to divide a circle.[12] Since ORESME will be concerned wholly with circular motion he has to show that propositions about the division of straight lines could be applied to circles.

The second proposition shows that if a given continuum is divided by two different geometric proportionalities there will be no points of division in common unless the numbers following the unit in the respective proportionalities are commensurable. Thus if we divide the continuum successively into 1, 2, 4, 8, 16, ..., equal parts and then divide it again successively into 1, 3, 9, 27, ..., equal parts there will be no points in common (except 1) between the two divisions because numbers 2 and 3 are prime to each other.

But if we divide the continuum by proportionalities 1, 3, 9, 27, ... and 1, 6, 36, 216, ..., respectively, there will be common points since 3 and 6 are commensurable. For example, points of division corresponding to $\frac{1}{3}$ and $\frac{2}{6}$ are common to both.

ORESME briefly mentions a concept which he will frequently use in later propositions. He observes[13] that if a given continuum is divided by a certain geometric proportionality such as 2^n or 3^n where $n = 1, 2, 3, \ldots \infty$, the continuum, in the successive divisions into smaller and smaller equal parts, ought to be exhausted. And yet this does not happen because we can divide it by numerous different proportionalities and yet still imagine that there are an infinite number of points in the continuum on which no point of division has yet fallen. Indeed we can divide the continuum by as many different proportionalities as we wish and if in each case the number following unity is a prime number none of these divisions will share common points.

[12] Two points on the circumference connected to the center of the circle by radii are required to divide the circle into two sectors.

[13] "Unde patet quod si esset taliter facta divisio in infinitum secundum proportionalitatem triplam, vel etiam duplam, nihil restaret dividendum. Et tamen contingit infinita puncta in quilibet nulla cecidit divisio" (Vat. lat. 4082, f. 98v, c. 1).

Proposition III

Dividendum continuum per fractiones phisicas quantumlibet impossible est prescindere partem, seu partes aliquotas, seu denominatas aliquo numero primo aut sibi multiplici preter 2 et 3 et 5.

However much a continuum be divided by physical (*i.e.*, sexagesimal) fractions, it is impossible to arrive at a part or aliquot parts denominated by some prime number, or multiple of some prime number, other than 2, 3, and 5.

ORESME, in this proposition, gives reasons why he will use vulgar rather than sexagesimal fractions to express the parts of any circle traversed by some mobile or mobiles.

Let us suppose that we divide a continuum by a sexagesimal proportionality. It is divided first into 60 equal parts, then into 60^2 equal parts, and so forth into $60^3, 60^4, \ldots, 60^n$ successive equal parts. Since 60, the second term in the proportionality after 1, has three prime numbers as factors, namely 2, 3, and 5, any fractional part of 60^n equal parts may be taken provided that the denominator of the fraction is either 2, 3, or 5, or any multiple of these. Thus $\frac{1}{2}$, $\frac{1}{3}$, or $\frac{1}{5}$ parts of 60^n equal parts of the divided continuum can be taken and an integral number of these parts obtained. For example, the fractions just mentioned yield 30, 20, and 12 parts of 60 respectively. But a $\frac{1}{9}$ part of 60 does not yield an integral number of equal unit parts of 60 and we must divide the continuum into 60^2 equal parts which will produce 400 equal parts.

If, however, we divide 60^n equal parts by fractional parts which are reciprocals of prime numbers other than 2, 3, and 5 no exact number of parts can be obtained. Now if in a sexagesimal division of a circle, a mobile should traverse some integral number of degrees plus $\frac{1}{7}$ of a degree its motion could not be precisely represented in the sexagesimal system. Indeed two motions which are commensurable might not have a precise common unit measure in a continuum which had been divided sexagesimally. For example, a mobile which traveled one degree in a day and another which traversed one and $\frac{1}{7}$ degrees have no precise measure in the sexagesimal system.[14]

ORESME emphasizes that this is not peculiar to the sexagesimal system for if we divide a circular or rectilinear continuum by one proportionality only the same problem arises, namely that certain fractional parts cannot be expressed by an exact number of equal parts into which the continuum is divided. If we divide a circle into 17 equal parts or signs and each sign into 17 degrees, and each degree into 17 minutes, and so on, then any fraction p/q, where q is an integer other than 17^n, will be inexpressible by an integral number of parts.

For these reasons ORESME wishes to avoid using any one particular proportionality and thus rejects the sexagesimal system, customarily employed in astronomical calculations where exactness is not expected, and decides to use vulgar fractions which, of course, embrace the whole range of rational fractions.

[14] Referring to the sexagesimal system ORESME says "non ergo sequitur si motus celi sint commensurabiles quod per tabulas factas possint commensurari precise, vel equari, quia possibile est quod unum mobile pertranseat in die unum gradum precise, et aliud mobile gradum cum septima parte unius gradus; vel etiam in die tertiam decimam partem unius gradus vel vicensimam secundam partem totius circuli aut secundum aliquem alium secundum quem non potest abscindi aliqua pars per divisionem tabularum communium" (Vat. lat. 4082, f. 98v, c. 2).

Thus, as will be seen, he can relate any such fractions by reducing them to a common denominator and consequently express all fractions involved in any particular example as parts of the common denominator. This is necessary because ORESME wishes to express exact punctual velocities and distances which would be impossible where only one proportionality is utilized[15] since that immediately restricts the domain of employable fractions to those whose denominators are exactly divisible by the term immediately following unity.

Proposition IV

Si duo mobilia nunc sint coniuncta necesse est ut alias in puncto eodem coniungantur.

If two mobiles should now be in conjunction it is necessary that they should conjunct in the same point at other times.

Mobiles moving with commensurable velocities will repeatedly conjunct in their present point of conjunction.

Let V_a and V_b represent respectively the velocities of two mobiles A and B. Then, by Euclid X, 5 it follows that $V_a/V_b = p/q$ where p and q are integers in their lowest terms. At the end of a certain time interval A will have completed p circulations and B will have moved through q circulations and they will again conjunct in their present point of conjunction. The same reasoning must apply to past conjunctions in the same point.

Proposition V

Tempus invenire quando primitus coniungentur in puncto in quo nunc sunt.

[How] to find the time when the two mobiles will first conjunct in the point in which they are now.

Having shown that two mobiles moved commensurably will conjunct repeatedly in their present point of conjunction, ORESME, in the fifth proposition, determines the time interval between two successive conjunctions in that same point. In other words, ORESME seeks the period of revolution for the two mobiles, and decides to use the day as his unit of time.

Using the day as time unit and assuming $V_a > V_b$, he says that if A completes one circulation in q days, B will require p days since $V_a/V_b = p/q$. Knowing that A will complete p circulations in q days, and B q circulations in p days, ORESME multiplies $p \cdot q$ to give the period of revolution in days. In his example $p/q = \frac{5}{3}$.

Proposition VI

Datis velocitatibus duorum mobilium nunc coniunctorum, tempus prime coniunctionis sequentis reperire.

[How] to find the time of the first conjunction following when the velocities of two mobiles now in conjunction have been given.

Assuming that the two mobiles are now in conjunction, ORESME finds the time for the very next conjunction at whatever point on the circle this may occur.

[15] "Verumtamen philosophi, compositores tabularum, non intendebant talem precisionem quia per nullas tabulas unius proportionalitatis posset haberi omnimoda precisio omnium motuum. Sed usi sunt divisione secundum proportionalitatem sexagintuplam quia ipsa est ad eorum intentionem aptissima. Nichilominus, in hoc libello, in quo loquendum est magis mathematice, oportet uti fractionibus omnino precisis que vocantur vulgares, quia iam ostensum est quod alius modus non sufficit adequandum omnem velocitatem precise" (Vat. lat. 4082, f. 98v, c. 2).

The first step requires that the circle be divided into a number of equal parts. In the particular case the number of parts is equal to the product $p \cdot q = 15$. Since $V_a > V_b$ there must be a conjunction when A, the quicker mobile, makes one more circulation than B and overtakes it. If A traverses $1/q = \frac{1}{3}$ of a circle in a day (it makes one circulation in 3 days), and B moves $1/p = \frac{1}{5}$ of a circle per day, the distance which A gains over B every day is $1/q - 1/p = p - q/pq$ or $\frac{2}{15}$ of a circle. Finally, by dividing the denominator by the numerator we get the time of the next conjunction. Thus, $pq/p - q = \frac{15}{2} = 7\frac{1}{2}$ days and at the end of that time A will have gained a full circle over B.[16]

Proposition VII

Datis duobus motibus duorum mobilium, numerum coniunctionum totius revolutionis invenire.

[How] to find the number of conjunctions in a complete revolution when the motions of the two mobiles have been given.

ORESME now explains how to find the total number of conjunctions which will occur during a period of revolution. The time between any two successive conjunctions is always equal because the velocities of the mobiles are taken as respectively constant. Hence it is only necessary to divide the period of revolution (found by Proposition V) by the time interval between any two successive conjunctions (Proposition VI). There will be two conjunctions in the previous example since $\frac{15}{7\frac{1}{2}} = 2$.

Proposition VIII

Datis duobus mobilibus nunc coniunctis, locum prime coniunctionis sequentis assignare.

[How] to determine the place of the first conjunction following the present conjunction of the two given mobiles.

In Proposition VI the *time* of the very next conjunction was found for two mobiles now in conjunction. In the eighth proposition ORESME shows how to find the *place* of that conjunction.

Finding the place depends upon knowing the distance which either of the mobiles travels per day and the time between the successive conjunctions. Thus, referring again to the previous example, A traverses $\frac{1}{3}$ and B $\frac{1}{5}$ of a circle per day and $7\frac{1}{2}$ days is the time which will elapse before the very next conjunction. Either the distance of A or B may be used. Using B we multiply $7\frac{1}{2} \cdot \frac{1}{5}$ and obtain $\frac{3}{2}$ which means that B has traveled $1\frac{1}{2}$ times around the circle in the time elapsed between the two conjunctions. Finally, it is necessary to subtract the whole circle from $1\frac{1}{2}$ and this leaves $\frac{1}{2}$. Hence A and B will conjunct in a place half way round the circle from their last place of conjunction, or directly opposite to it.

[16] At the end of the conclusion ORESME summarizes the rule for determining the next conjunction: (1) subtract the motions—*i.e.*, the distances traversed—and (2) divide the numerator of the difference into the denominator. The text reads: "Est ergo regula talis ad inveniendum propositum. Subtrahatur motus unius a motu alterius et residuum habet numeratorem et denominatorem. Dividatur itaque denominator per numeratorem et exibit tempus quesitum" (Vat. lat. 4082, f. 99v, c. 1).

Expressed in terms of the letters p and q we have for mobile A $pq/p-q \times 1/q = p/p-q$; and for B we have $pq/p-q \cdot 1/p = q/p-q$. Where it happens that the division of $(p-q)$ into either p or q produces a quotient consisting of an integer plus a fraction it only remains to eliminate the integer. The fraction alone reveals the part of the circle separating the two successive points of conjunction and hence locates the next point of conjunction.[17]

Proposition IX

Assignata distantia duorum mobilium, locum et tempus prime coniunctionis sequentis dare.	[How] to find the place and time of the next conjunction following when the distances of the two mobiles have been assigned.

In previous conclusions the conjunctions and motions of mobiles A and B were calculated from a present conjunction. But now, after having devoted separate conclusions to a determination of the *time* (Proposition VI) and then the *place* (Proposition VIII) of the first conjunction after departure from the present point of conjunction, ORESME, in Proposition IX, considers how to calculate *both the time and place* of the first conjunction *when there is a given distance separating the mobiles.*

ORESME decides that the distance between any two mobiles is to be calculated from the slower mobile counterclockwise to the quicker mobile. Thus if we suppose there are twelve signs in a circle and that A is quicker than B, then if A is one sign ahead of B clockwise the distance separating them counterclockwise would be eleven signs. Or, simply, B is eleven signs ahead of A.[18]

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[17] ORESME expresses the elimination of the integer by saying that the whole circle should be subtracted from the total distance traversed between the two successive conjunctions and asserts that this is the same as dividing the whole circle by the distance traversed presumably because the fraction remaining will divide the circle. In the example cited A has traveled $\frac{5}{2}$ and B $\frac{3}{2}$ times around the circle so that A overtakes B after completing one more turn around the circle. Since A has traveled $2\frac{1}{2}$ times around the circle we can subtract 2 from $2\frac{1}{2}$ or, in the case of B, 1 from $1\frac{1}{2}$. We have now divided the circle into halves.

The text reads: "Deinde ab illo pertransito subtrahatur totus circulus quotiens poterit subtrahi, si est possibilis talis subtractio, et hoc est idem quod dividere totum circulum per illud pertransitum et habebitur propositum" (Vat. lat. 4082, f. 99v, c. 1).

[18] "Hec distantia signanda est secundum circuli portionem incipiendo a velociori ita quod mobile velocius ponatur retro. Unde si A precederet B per unum arcum parvum, ut per unum signum, tunc B diceretur ante A per residuam circuli portionem, scilicet per undecima signa" (Vat. lat. 4082, f. 99v, c. 1). My introduction of the terms "clockwise" and "counterclockwise" seems to depict accurately ORESME's intent. His choice of the slowest moving mobile as his point of reference to express distance relationships seems motivated by a desire to have the quicker moving body conceived of as closing a gap between itself and a slower mobile. Thus immediately after a conjunction with a slower mobile, the quicker mobile passes it and is then 11^+ signs behind and will continually close the gap. The quicker mobile—except in conjunction—is always taken to be behind the slower, moving constantly "forward" and diminishing the distance between them until the next conjunction. This apparently seemed conceptually more "natural" than, for example, supposing that as the swifter passes the slower it is 0^+ signs distant and would constantly increase the gap to 11^+ signs prior to the next conjunction. From this standpoint the swifter would overtake the slower when it is farthest removed from it.

Once again the proportion of velocities is expressed as $V_a/V_b=p/q$ where V_a is the velocity of mobile A and V_b that of B and where p and q are a ratio of numbers prime to each other with $p>q$. Two cases are treated by ORESME.

In the first case the difference between the numbers representing the ratio of velocities is equal to the distance separating the mobiles. Thus $p-q=D_{B\to A}$ where $D_{B\to A}$ is the distance separating A and B measured counterclockwise from B to A and expressed positively in either degrees, signs, or some other unit of measure. When these conditions obtain, A and B will conjunct when A moves p and B moves q signs or degrees. In his example, ORESME sets $V_a/V_b=\frac{8}{3}$ and $D_{B\to A}=5$ degrees. Therefore when A moves 8 degrees, B will move 3 degrees and they will conjunct.

If however, —and this is the second case—, $p-q\neq D_{B\to A}$ the following proportional relationship will determine the distances which must be traveled for the conjunction to occur: $p-q/D_{B\to A}=p/z$ or q/z, where z is the unknown distance which either p or q must traverse in order to conjunct. Now if the ratio of velocities is again $p/q=\frac{8}{3}$ but now $D_{B\to A}$ is 2 degrees, we introduce only p/z from which we can find z, the distance which A must travel to conjunct with B. Thus $(8-3)/2=8/z$ and z equals $3\frac{1}{5}$ degrees. By substituting q/z for p/z it is found that B must travel $1\frac{1}{5}$ degrees to conjunct with A.

Although ORESME has not actually specified the time and place of conjunction these could be easily calculated from Propositions VI and VIII respectively. In this proposition he concentrated solely on the problems arising in calculating a future conjunction when initially A and B are separated rather than in conjunction.

Proposition X

Numerum et seriem punctorum reperire in quibus umquam talia duo mobilia coniungentur.	[How] to find the number and sequence of points in which two such mobiles will always conjunct.

In this proposition ORESME describes how to determine the ***number and order*** of the points of conjunction for any two mobiles.

By Proposition VII the total number of conjunctions in a period of revolution can be ascertained. This coupled with the fact that the times between any two successive conjunctions are equal (since the velocities, though different, are respectively uniform) dictates that the number of conjunctions in a period of revolution equals the number of distinct places or points of conjunction. These distinct points of conjunction must be equidistant because the times between successive conjunctions are equal. Hence the points of conjunction divide the circle into a number of parts equal to the number of points of conjunction. If there are five conjunctions in a revolution there will be five different points of conjunction dividing the circle into five equal parts. Conjunctions can occur only in these five points.

In Proposition XI ORESME presents a simple method for finding the total number of points of conjunction of two mobiles during every revolution. But in Proposition X, he simply assumes this in order to show the order of the points of conjunction.

Let $V_a/V_b = \frac{12}{5}$[19] so that the difference of the velocities is $12-5=7$ and the number of distinct points of conjunction is 7 (shown in the next proposition). By Proposition VIII one can show that any conjunction occurs $\frac{5}{7}$ of a circle away from the immediately preceding conjunction.[20] Knowing the number of points of conjunction and the distance separating any two successive conjunctions, we can now arrange them sequentially. When a conjunction occurs in any point, say C, the next conjunction must occur $\frac{5}{7}$ of a circle away from C. The circle can be divided into 7 equidistant points numbered clockwise from C_1 to C_7. Assuming the first conjunction of a period of revolution to occur in C_1 the second conjunction must occur in point C_6 which is $\frac{5}{7}$ of the circle from C_1. The third conjunction will be C_4 and the remaining four conjunctions are, in order of occurrence, C_2, C_7, C_5, C_3. The cycle is then repeated beginning with C_1.[21]

Proposition XI

Omnium duorum mobilium tot sunt coniunctiones in una revolutione et tot puncta in quibus umquam possunt coniungi, quota est differentia minimorum numerorum proportionis velocitatum motuum.

The number of conjunctions of any two mobiles in one revolution, and the number of points in which they can conjunct, equals the difference between the numbers representing the proportion of their velocities, when those numbers have been reduced to their lowest terms.

Here ORESME presents a simplified version of Proposition VII for determining the number of points of conjunction in the course of one complete period of revolution. He shows that the number of points of conjunction equals the difference between the integers representing the ratio of velocities. Thus if $V_a/V_b = p/q$, with p and q mutually prime and $p>q$, then $p-q=n$ where n represents the total number of points of conjunction in every revolution of mobiles A and B.

In Proposition IV[22] it was shown that when A and B complete p and q circulations respectively, they will conjunct in the point in which they are now—*i.e.*,

[19] In Proposition XI, ORESME demonstrates that $V_a - V_b = n$, where n is the number of points of conjunction. Since he is using this demonstration in Proposition X, he assigns a definite numerical velocity to each mobile. But on the basis of previous propositions and ORESME's customary usage, it must be understood that he is thinking of a *ratio* of velocities which can be related as two numbers reduced to their lowest terms.

The actual language of the text reads: "Sit velocitas A sicut 12, et velocitas B sicut 5 ..." (Vat. lat. 4082, f. 99v, c. 2).

[20] In Proposition XI the period of revolution is shown to be 60 days. Since there are 7 points of conjunction the time between successive conjunctions is 60/7, or $8\frac{4}{7}$ days. Mobile A traverses $\frac{1}{5}$ of its circle per day and therefore in $8\frac{4}{7}$ days will make $1\frac{5}{7}$ circulations. Mobile B, traveling $\frac{1}{12}$ of its circle per day covers $\frac{5}{7}$ of a circulation in $8\frac{4}{7}$ days. After $8\frac{4}{7}$ days A will make one more circulation than B and conjunct with it $\frac{5}{7}$ of a circle away from the last point of conjunction.

[21] The pertinent text for the example cited above is as follows: "Sit velocitas A sicut 12 et velocitas B sicut 5. Tunc per istam conclusionem, et etiam per sequentem, invenietur quod numerus punctorum in quibus A et B umquam coniungentur est 7, et per octavam conclusionem reperitur quod unaqueque coniunctio distat localiter ab ultimo puncto per $\frac{5}{7}$ circuli. Ergo cum sint septem puncta circuli equaliter ab invicem distantia erit coniunctio in uno, deinde in sexto, ab isto, scilicet quatuor punctis intermissis; deinde in sexto ab isto aliis quatuor intermissis, et sic semper. Et in aliquo casu obmitterentur duo, vel tria, et quandoque nullum. Sed sive saltum coniunctiones fierent ordinate per hec puncta" (Vat. lat. 4082, f. 99v, c. 2—100r, c. 1).

[22] The manuscripts cite Proposition 5 which is inappropriate since it determines only the period of revolution. I have substituted Proposition 4 which is genuinely applicable.

will have completed a revolution. When this occurs A will have completed $p-q=n$ more circulations than B. Now every time A gains one circulation over B there must be a conjunction, from which it follows that A and B must conjunct n times since A has gained n circulations.

For example, if as before, $V_a/V_b=\frac{5}{3}$ then in one revolution A and B will conjunct twice. The first conjunction will occur in the point opposite their present point of conjunction when A makes $2\frac{1}{2}$ and B $1\frac{1}{2}$ circulations. The second conjunction will be in the present point when A completes 5 and B 3 circulations.

After a five-step summary of the procedures leading to the determination of the order in which conjunctions occur[23], we find an interesting application of Proposition XI to the widely held view that the planets move with velocities which are related in harmonic proportions[24] and thus produce the celestial harmonies. ORESME observes that the successive terms in harmonic proportions are related as $(n+1)/n$, where $n=1, 2, 3$, and therefore the difference of velocities would always be 1. Now if this be true then Proposition XI can be legitimately applied and it would follow that any two planets with velocities related as one of the principal harmonic proportions can conjunct in only one point and nowhere else. Since this is contrary to experience one may conclude that no two celestial motions are related as any of the principal harmonic motions, although it is possible that celestial bodies may produce consonances by virtue of something other than their ratios of velocities.[25]

By granting the basic assumptions of the celestial harmony theory, ORESME has drawn from it an empirically testable consequence which is contrary to observation. Thus any celestial harmony theory which supposes that the motions of any two planets are related commensurably by harmonic ratios is untenable.

Proposition XII

Si fuerint mobilia plura duobus possibile est quod numquam coniungentur simul plura quam duo.

If there should be more than two mobiles, it is possible that no more than two will ever be in conjunction at the same time.

Commencing with Proposition XII, there follows a series of propositions involving three or more mobiles in motion simultaneously.

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[23] This summary is simply a concise formulation of earlier propositions pertinent to a proper ordering of conjunctions in any period of revolution.

[24] The ratios mentioned are the dyapason ($\frac{2}{1}$ or octave), dyapente ($\frac{3}{2}$ or fifth), the dyatesseron ($\frac{4}{3}$ or fourth). For a table of the harmonic intervals see G. FRIEDLEIN (ed.), BOETII, De Institutione Arithmetica libri duo, De Institutione Musica libri quinque (Lipsiae, 1867), p. 201. A brief account of the Pythagorean theory of celestial harmony is given by THOMAS L. HEATH, A History of Greek Mathematics (Oxford, 1921), vol. I, p. 165.

[25] ORESME also mentions the dyesis or semitone, namely 256/243, where the difference is 13 and consequently only 13 places of conjunction are possible.

The pertinent passage is as follows: "Cum igitur non inveniatur in motibus celestibus quod ex duobus motibus oriatur [coniunctio] (the manuscripts have *constellatio*) que non possit fieri nisi in uno puncto, tunc consequens est ut nulli duo motus celestes in velocitate teneant proportionem armonicam principalem. Et ergo si corpora celestia faciant consonantiam in movendo non oportet quod ex velocitatibus motuum proveniat huiusmodi consonantia sed potest aliunde oriri ut postea videbitur per alias rationes" (Vat. lat. 4082, f. 100r, c. 2).

In Proposition XII it is shown that under certain conditions it is possible that only two of the three or more mobiles can ever conjunct at the same time. In treating these propositions, ORESME takes only two mobiles at a time. Let us suppose there are three mobiles, A, B, and C. By previous conclusions we know that A and B can conjunct in a limited number of different points and in no others. Let d represent any one of these points. Similarly, B and C can only conjunct in a limited number of points, any one of which may be represented by e. If it can be shown that no d is an e it follows that the three mobiles will never simultaneously conjunct. In the same manner if there are four, five, or any number of mobiles, it is possible that when taken two at a time the points of conjunction of any two do not serve as points of conjunction for any other two mobiles. In this event no more than two mobiles can conjunct simultaneously in the same point.

If there were six mobiles, and the conditions outlined above obtained, no more than two could ever conjunct at the same time and place. But it would be possible that three, four, five, or even all six might conjunct simultaneously in the same point if, when taken two at a time, they shared some or all points of conjunction.

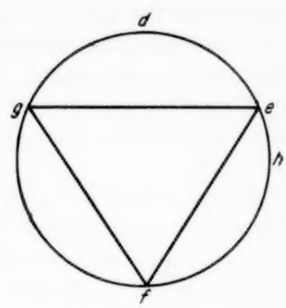

Fig. 1 [26]

ORESME furnishes an example for three mobiles which can never conjunct. The mobiles A, B, and C may be combined into three pairs, namely A and B, A and C, and B and C. Let the ratios of velocities be $V_a/V_b=\frac{4}{2}$, $V_a/V_c=\frac{4}{1}$, and $V_b/V_c=\frac{2}{1}$. In Figure 1 point e is distant from d by two signs, or $\frac{1}{6}$ of a circle, and points f and g together with e divide the circle into three equal parts. Finally, point h is three signs distant from d. At the outset A and B are in conjunction in d and C precedes A and B by $1\frac{1}{2}$ signs (*i.e.*, by $\frac{1}{8}$ of the angular measure of the circle).

By Proposition IX and their ratio of velocities A and C will conjunct in e because C will traverse $\frac{1}{2}$ a sign while A moves 2 signs from d to e. The only points of conjunction for A and C are in e, f, and g, which equally divide the circle into parts of four signs each. This is obvious because from conjunction in e, A will traverse 16 signs ($1\frac{1}{3}$ circles) and C 4 signs ($\frac{1}{3}$ of a circle) which brings them to f, and then to g. This will be repeated ***ad infinitum***.

Mobiles A and B can conjunct only in d where they are initially in conjunction. The difference of their velocities when reduced to their lowest terms is 1 and d is therefore the only point in which they can ever conjunct (Proposition XI).

Mobiles B and C can conjunct only in point h. Since C precedes B by $1\frac{1}{2}$ signs and their ratio of velocities is $\frac{2}{1}$, they will conjunct in h which is 3 signs from d. Thereafter B and C can conjunct only in point h.

From all this we see that the only points of conjunction are d, e, h, f, and g but in none of these points can A, B, and C conjunct simultaneously.

[26] This figure appears in the margin of Vat. lat. 4082, f. 100v, c. 1.

Proposition XIII

Omnium trium aut plurium mobilium que numquam simul coniungentur est certa distantia citra quam approximari non possunt.	Of any three or more mobiles which will never be simultaneously in conjunction, there is a certain [minimum] distance below which they can not approximate to each other.

Since the three mobiles in the preceding proposition will never conjunct, ORESME considers next the smallest possible space which will encompass them. That is, if A, B, and C can never conjunct, how close together can they possibly come short of a conjunction.

Recalling from Proposition XII that $V_a > V_b > V_c$, it is demonstrated that only when the quickest and slowest of the mobiles, namely A and C, are in conjunction can all three be "squeezed" within a minimum possible space. ORESME distinguishes two cases: (1) when A and C are in conjunction and B precedes them, and (2) when A and C are in conjunction and B follows. The minimum space can be achieved in either case, and in order to demonstrate this ORESME simply eliminates the other possibilities.

Excluding conjunctions, it is clear that only six sequential arrangements of the mobiles are possible. These are:[27]

(1)	A	B	C
(2)	A	C	B
(3)	B	A	C
(4)	B	C	A
(5)	C	A	B
(6)	C	B	A

The mobiles represented by the letters in the extreme right hand column are to be taken as the "preceding" or lead mobiles, the letters in the center column as the mobiles in the center, and the extreme left column as the rear mobiles. Thus in (2) B precedes A and C, while C is in the middle with A bringing up the rear and moving toward conjunction with C.

ORESME now moves to eliminate all six possibilities. He considers first the distance separating B from A and C, respectively, ***immediately before and after*** conjunction of A and C. Thus, in (2) above, when B precedes both A and C it will be more distant from A, the rear mobile, ***immediately before*** conjunction than during A's conjunction with C. It follows that A, B, and C are more widely spaced before conjunction than during conjunction and (2) cannot be a minimum distance encompassing all three mobiles. Now immediately after conjunction B will be further removed from C than during conjunction of A and C.[28] This follows

[27] This arrangement of mobiles is based on a figure which appears in the lower margin of Vat. lat. 4082, f. 100v, c. 2.

[28] In the comparisons of distance, the previously mentioned method of measuring distances counterclockwise from the slowest to the quickest mobile seems to be ignored by ORESME in cases (4), (5), and (6). In all six cases the distance of separation must be measured between the extreme mobiles—that is between the first, or preceding mobile and the last, or following mobile. However, in cases (4), (5), and (6) the quicker

from the fact that $V_b > V_c$ and consequently (5) is eliminated as a candidate for minimum distance.

When, however, B follows A and C, it will happen that B is more distant from A *immediately after* conjunction (rather than immediately before as when B preceded A and C in case (2)) than when A and C were actually in conjunction. This is determined by the fact that $V_a > V_b$, and consequently (4) is eliminated. Case (3) is rejected because B will be more distant from C *immediately before* conjunction than during conjunction of A and C. This is clear since $V_b > V_c$.

Up to this point ORESME has only eliminated (2), (5), (4), and (3). But he then supposes that someone might agree that the four possibilities already cited should be rejected and yet still deny that A and C must be in conjunction as a necessary condition for achieving a minimum distance for the three mobiles.[29] This person must then opt for (6), namely where C is behind B and A precedes both. In that event it is clear that the mobiles would have been even closer when A was in conjunction with B and even closer, indeed closest, when A was, or will be in conjunction with C. Disposition (6) is hopeless because the order of the mobiles is such that the quickest mobile A, precedes and the slowest, C, is last which means that the distance between A and C constantly increases the instant they enter this disposition.

Disposition (1) is not mentioned, presumably because it is superfluous and can be reduced to (3). Clearly the mobiles will be closer after A passes B than before. But when this occurs (1) converts to (3).

Satisfied that he has eliminated all possibilities, ORESME furnishes an example to illustrate that the minimum condition for the three mobiles to be embraced within the smallest possible space is that A and C, the quickest and slowest mobiles, be in conjunction with B either preceding or following. To do this he uses the data and figure from the preceding proposition.

Recalling that A and B conjunct in d and that C precedes them by $1\frac{1}{2}$ signs, we saw that A and C (where $V_a/V_c = \frac{4}{1}$) will then conjunct in e, two signs away from d. But in the same time B has moved only one sign from d because its

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mobile precedes and the slower follows. Therefore, by measuring counterclockwise from the slower to the quicker mobile, according to ORESME's procedure, we arrive at greater distances of separation than if we measured clockwise from slower to quicker, or counterclockwise from quicker to slower. This is evident, for example, in case (5), when ORESME says that B will be more distant from C immediately after conjunction than when C was in conjunction. This would be false if the distance between B and C were measured from C, the slower mobile, counterclockwise to B. In that event the distance would diminish as B gains on C and immediately after conjunction between A and C, mobile B will be a smaller distance from C than when C was in conjunction. In cases (1), (2), and (3) ORESME's procedure will produce the required results.

ORESME has, therefore, abandoned his rule—perhaps unknowingly—and simply taken the shortest possible absolute distance between any two extreme mobiles independently of whether this entails measuring clockwise or counterclockwise from the slowest mobile. This seems to have been thrust upon him by the very proposition itself since the objective is to determine minimum possible distances between mobiles.

[29] "Quid si negetur arguitur adhuc aliter. Et primo ponatur quod A precedat, et B sequitur, postea C. Igitur erant propinquiora quando A coniungebatur cum B, et adhuc propinquiora quando A coniungebatur cum C vel quando coniungetur cum ipso, ut statim patet ex ordine velocitatum" (Vat. lat. 4082, f. 100v, c. 2).

velocity is half of A's. At this juncture only one sign separates B from A and C in conjunction at e. ORESME goes on to show that this is, indeed, the minimum space into which the three mobiles can be crowded; or to put it another way, the closest they can come to conjunction.

Applying all this to planetary motions, ORESME conjectures that just as with the mobiles, it might happen that three or four planets moving commensurably with respect to one another might never conjunct, though they might come within two or three degrees of conjunction.[30]

Proposition XIV

Si plura mobilia nunc sint coniuncta necesse est ut in puncto eodem alias coniungantur.

If several (*i.e.*, three or more) mobiles should now be in conjunction it is necessary that they should conjunct in that same point at other times.

Proposition XV

Quando hoc primo fiet invenire.

[How] to find when the several mobiles would first conjunct again at the point in which they are now in conjunction.

Proposition XVI

Tempus reperire in quo huiusmodi mobilia sive in puncto in quo nunc sunt, sive in alio primitus coniungentur.

[How] to find the time in which such mobiles will conjunct first, whether that conjunction be in the point in which they are now, or in some other point.

Proposition XVII

Coniunctiones totius revolutionis seu totius periodi numerare.

[How] to number the conjunctions of a whole revolution or period.

Proposition XVIII

Locum prime coniunctionis sequentis assignare.

[How] to determine the place of the first conjunction following [the present conjunction].

Proposition XIX

Numerum et seriem punctorum reperire in quibus umquam talia plura mobilia coniungentur.

[How] to find the number and sequence of points in which several (three or more) such mobiles will always conjunct.

In Propositions XIV through XIX, ORESME extends to three or more mobiles results which had been demonstrated previously for two mobiles only. This is easily accomplished because where three or more are involved they are taken two at a time.

The direct correspondence between the later and earlier propositions is as follows: XIV and IV, XV and V, XVI and VI, XVII and VII, XVIII and VIII, XIX and X.

[30] "Possibile est ergo quod aliqui tres planete, vel quatuor, numquam coniungentur in eodem gradu, vel in eodem minuto, et forte de aliquot possibile est quod non possunt appropinquari quin distent per duos gradus, vel tres, si omnes commensurabiliter moveantur" (Vat. lat. 4082, f. 100v, c. 2).

Proposition XX

Si circuli fuerint eccentrici erit idem numerus locorum qui esset si forent concentrici, sed erunt distantie temporis et spatii inequales.

If the circles should be eccentric the number of places (of conjunction) would be the same as if they were concentric, but the intervals of time (between conjunctions) and the spaces will be unequal.

Up to this point the motions of the mobiles have been assumed to take place on concentric circles, but in Proposition XX ORESME supposes the mobiles to move on eccentric circles.

In the enunciation of this proposition ORESME asserts that the *number* of places of conjunction for a given set of mobiles moving commensurably would be the same whether the circles are eccentric or concentric, but the eccentricity affects the time and distance intervals between successive conjunctions.

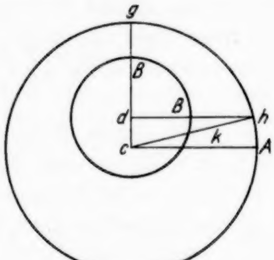

Fig. 2[32].

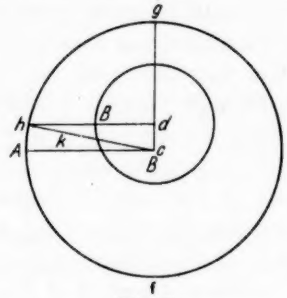

Fig. 3[34]

Since the motions of the mobiles on the eccentric circles are assumed commensurable, the following results of previous propositions will apply: (1) mobiles in conjunction in a particular point must have been in conjunction in the same place at other times; (2) the number of such points of conjunction is finite; (3) the conjunctions repeat themselves exactly after every period of revolution.[31]

In Fig. 2, *A* and *B* are mobiles moving on their respective circles with velocities initially unexpressed, but actually related as $\frac{5}{1}$ (see below); *c* is the center of the world and center of *A*'s motion; *d* is the center of *B*'s motion. At the outset let us suppose that *A* and *B* are in conjunction on line *cdg* which serves as an aux line[33], and that line *dh* is a quarter of the circle away from line *cdg* on *B*'s circle, and line *ck* is distant a quarter of the circle from line *cdg* on *A*'s circle.

[31] "Ob hoc enim quod motus sunt commensurabiles necesse est quando mobilia sunt in uno loco coniuncta quod ibidem alias coniungantur, ut prius probatum est; ergo loca talia sunt finita et facta revolutione omnium coniunctiones iterum incipiunt fieri sicut ante" (Vat. lat. 4082, f. 101 v, c. 2).

[32] Figure 2 appears in Vat. lat. 4082, f. 101 v, c. 2 lower margin. Line *cdg* is not drawn in the manuscript.

[33] The aux *(Lat. aux, augis)* point is equivalent to the apogee of a planet, and the line of aux connects its apogee and perigee *(oppositum augis)*. In the particular case under discussion, the line of aux must be restricted to mobile *B* on the eccentric circle, for it is vacuous to say that mobile *A* has a point of apogee and perigee when it moves on a circle whose center is the center of the world, since *A* will always be equidistant from the center of the world. For references to a discussion of aux and aux line see DEREK J. PRICE, The Equatorie of the Planetis (Cambridge Univ. Press,

ORESME notes that if the circles were concentric the mean and true motions of the mobiles would be the same. Hence if B traversed $\frac{1}{4}$ part of its circle it would reach line *dh*, and if, in the same time, A should traverse its circle $1\frac{1}{4}$ times it would reach line *ck* and conjunct with B. But the circles are eccentric, not concentric, and because A's velocity is greater than B's, they will conjunct *before* A moves $1\frac{1}{4}$ times around its circle and B $\frac{1}{4}$ the way around its circle. With respect to c, the center of the world, conjunction will occur repeatedly at some point, or line, closer to g.

Now, although the first conjunction after g did not occur at a point a quarter way around for each circle because of the eccentricity, the next conjunction will occur directly opposite g in point f (see Fig. 3). Although ORESME omits any discussion of the conjunction in f, it is clear the second conjunction must occur in f when it is recalled that in the time B moves $\frac{1}{4}$ of its circle, A will have moved $1\frac{1}{4}$ times around its circle. The first time this happened A and B had already had a conjunction, but now the same calculations from their respective quarter points show that they will conjunct in f after A traverses its circle $1\frac{1}{4}$ times and B moves through another $\frac{1}{4}$ of its circle.

Assuming a conjunction in f, the very next, or third, point of conjunction will not occur $\frac{1}{4}$ of the way around the two circles from f, but beyond that point somewhere in the last quadrant. This is evident because calculations from f reveal that the situation in the fourth quadrant of the respective circles will be the reverse of what happened earlier in the first quadrant (see Fig. 3). When B and A traverse $\frac{1}{4}$ and $1\frac{1}{4}$ of their respective circles from f, B, the slower mobile, precedes A, the faster. Hence they will not yet have had their next conjunction which will occur later at some other point in the fourth quadrant. The third conjunction, therefore, occurs later than if the circle were concentric. The fourth, and final, conjunction will occur once again in g.

In the example above, ORESME has shown that the number of conjunctions would be four, whether the circles are concentric or eccentric. But in eccentric circles the points of conjunction are not equally spaced and, since the motions remain respectively uniform, the time between any two successive conjunctions will not be equal. Two conjunctions occur at opposite points of the diameter gf, but the other two will take place in the first and fourth quadrants while none occur in the second and third. ORESME has thus demonstrated his proposition. Later, he notes that if the conjunctions were restricted to the aux point and its opposite (*i.e.*, g and f), then distance and time intervals would be equal between successive conjunctions—despite the eccentricity of the circles.[35]

1955), p. 207 (general index), and the definition of *aux* (p. 168) and *line of aux* (pp. 174–175) in the glossary of terms. See also SACROBOSCO's definition in LYNN THORNDIKE (ed. and tr.), The Sphere of Sacrobosco (Chicago, 1949), p. 140.

[34] This figure does not appear in the manuscripts but is clearly described. It has been added for convenience.

[35] Concerning motion on eccentric circles and the aux and its opposite, the text reads: "... quod distantie temporis et spatii aliter sunt quam si motus essent concentrici. Et si essent concentrici tunc forent equales distantie utrobique propter regularitatem motuum. Ergo nunc [referring to eccentric motions] sunt huiusmodi distantie temporis et spatii inequales, et hoc est verum nisi in casu ubi non fierent coniunctiones nisi in auge et in opposito augis" (Vat. lat. 4082, f. 102r, c. 1).

Clearly, then, mobiles with different but respectively uniform velocities will conjunct differently on concentric and eccentric circles. ORESME attributes this to the fact that on concentric—but not eccentric—circles the mean and true motions are identical.[36] This is explained by noting that when the mobiles are in conjunction at *g*, the mean motion would take longer to produce the first conjunction (or, as ORESME expresses it, the mean motion "adds" to the true motion) if the circles are concentric, rather than eccentric, with respect to *c*. But if the mobiles were moving toward their next conjunction from *f* (Fig. 3), the mean motion would produce a conjunction more quickly (ORESME says the true motion "adds" to the mean motion) with respect to *c* when the circles are concentric than when eccentric.[37] ORESME has shown that one is compelled to distinguish between mean and true motions when eccentric circles are considered and the motions are referred to the center of the world—*i.e.*, the earth.

Proposition XXI

Quecumque dicta sunt de coniunctione duorum vel plurium mobilium consimiliter intelligenda sunt de oppositione et de quocumque alio aspectu, sive modo, se habendi.	It must be understood that anything said about the conjunction of two or more mobiles also applies to opposition and to any other aspect in which the mobiles can be related.

Of all the aspects, only conjunctions of two or more mobiles have thus far been considered. In Proposition XXI the demonstrations about conjunctions are extended to embrace every astronomical aspect.

Apart from conjunction and opposition, ORESME distinguishes three other aspects which remain nameless. He probably meant *sextilis*, *quartilis* and *trinus*, where any two signs of the zodiac, or planets in those signs, are separated by two, three, and four signs of the zodiac respectively.[38]

Of these aspects, conjunction and opposition can only occur in one way, whereas the remaining three have a double character since each can happen either before or after a conjunction or opposition.[39] By this ORESME means that

[36] Although mean and true motions are identical for concentric motions, ORESME emphasizes that prior to Proposition XX all motions were treated as mean motions. In the present proposition, however, it is shown that many of the previous propositions are also applicable to cases involving true motions as in eccentric and epicyclic motions. "Omnia, itaque, dicta ante istam conclusionem sunt ad motus medios referenda. Et hec conclusio docet ad motus veros cuncta conformiter applicare non obstantibus eccentricis nec etiam epiciclis" (Vat. lat. 4082, f. 102r, c. 1).

[37] "... ita quod motus medius qui ymaginatur a *c*, si esset concentricus, ab una parte dyametri addit supra motum verum, et ab alia motus verus addit supra medium ..." (Vat. lat. 4082, f. 101v, c. 2—102r, c. 1).

[38] A discussion of these aspects appears in ROBERT ANGLICUS' Commentary on the Sphere of SACROBOSCO. See THORNDIKE (ed.), *op. cit.*, p. 176 for Latin text and p. 226 for the English translation.

[39] This brief proposition may be quoted *in toto*: "Quecumque dicta sunt de coniunctione duorum vel plurium mobilium consimiliter intelligenda sunt de oppositione et de quocumque alio aspectu sive modo se habendi. Verumtamen, distinguendus est aspectus trinus ante coniunctionem ab aspectu trino ipsam coniunctionem sequenti et sic de quolibet aspectu seu modo se habendi exceptis coniunctione et oppositione, quoniam omnis alter aspectus est dupliciter, scilicet ante coniunctionem et post, una vice a dextris et alia a sinistris. Quibus sic intellectis, omnia predicta possent de

with respect to a fixed point of conjunction two (or more) mobiles will proceed through the same aspects before and after conjunction, but the order of the aspects and mobiles after conjunction are reversed with respect to those before conjunction. Since prior to a conjunction the swifter mobile must overtake the slower, the order of the aspects will be successively trinal (separated by four signs), quartile (separated by three signs), and finally sextile (separated by two signs). But after conjunction the order is reversed and will be sextile, quartile, and trinal as the faster mobile moves away from the slower. Before conjunction the slower mobile precedes while after conjunction it follows, creating alternately narrowing and widening intervals of space as the swifter mobile overtakes and then leaves it behind.

With this in mind it is clear that the previous propositions apply to aspects of both concentric and eccentric circles. ORESME does not deal separately with the two types of circles in this very brief conclusion, but in eccentric circles the same reservations which were previously applied to conjunction must now be extended to all other aspects. That is, just as the points of conjunction are not equally spaced so the other aspects will not be equally spaced, and the same applies to the time intervals. On concentric circles all aspects will repeat in definite positions with respect to the finite number of fixed points of conjunction, and the time intervals between successive occurrences of some given aspect will always be equal.

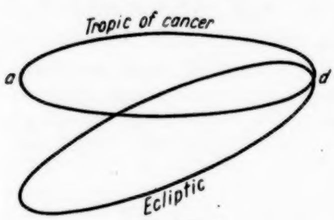

Fig. 4 [40]

Proposition XXII

Consimilia applicare ad idem mobile quod pluribus motibus moveretur.

[How] to apply to one and the same mobile moved with several [simultaneous] motions propositions similar [to those previously demonstrated for two or more distinct mobiles].

Heretofore every mobile was taken to have a unique motion, but in Proposition XXII, ORESME investigates the case of a single mobile which can be assigned several motions simultaneously.

The double motion of the sun—diurnal and annual—serves as the basic illustration. In Fig. 4 circle a is the tropic of Cancer (summer tropic) which describes daily a complete circulation. Let A_n (not shown in the figure), where n may be any integer, be the first point of Cancer (the summer solstice) on circle a. Let B be the center of the Sun which describes the ecliptic with a uniform motion in a year. Point d is imagined fixed in space and is the only point of contact between circle a and the ecliptic. Therefore any point, A_n, on circle a[41] will

quolibet aspectu omnino similiter demonstrari sicut iam de coniunctionibus probata sunt" (Vat. lat. 4082, f. 102r, c. 1). The term "aspectus trinus" refers to the three kinds of aspects, namely sextile, quartile, and trinal.

[40] The figure appears in Vat. lat. 4082, 102r, c. 1 in the lower margin. I have slightly altered both the figure and its position.

[41] ORESME speaks of a as the first point of Cancer, but from the context it is clear that *circle a* is meant rather than some specific point a. Thus the first point of Cancer

serve as the first point of Cancer when it is in *d* simultaneously with *B*, the center of the Sun.

At the outset, let us suppose that A_1 and B are in *d*. Since the motions of A_1 and B are assumed commensurable, their proportion of motion is rational. If this proportion of motion is multiple, namely $n/1$, where n is any integer, B will always meet the same point A_1 in *d*. Thus if circle *a*, bearing point A_1, should make 100 circulations while B made only one, B would never meet any point other than A_1 in *d*.

If, however, the proportion of motions is not multiple, but n is an integer plus a fraction, then the denominator of the fraction will indicate the number of different points of circle *a* which B could meet in *d*. For example, should circle *a* complete $100\frac{1}{2}$ circulations to one for B there would be two fixed points, A_1 and A_2, which B could meet in *d*. This is easily seen if A_1 and B are now in *d*. After $100\frac{1}{2}$ circulations of circle *a* point A_1 will be opposite *d* since it will have gone $\frac{1}{2}$ a circle beyond *d*. Another point A_2 will therefore meet B in *d*. Following the next $100\frac{1}{2}$ circulations, A_1 will once again meet B in *d*, and A_2 will now be opposite *d*. This pattern will continue *ad infinitum*.

In general, the total number of points on circle *a* which can serve as first point of Cancer equals n in the fraction $P\ m/n$, where P is an integral number of circulations and m/n represents an additional fractional part of a circulation with $m < n$ and both are integers. The exact order of the points through which the Sun can enter the first point of Cancer can now be determined, and this can be done for any other point or degree of the zodiac.

From this analysis, ORESME concludes that if the solar year were measured by an integral number of days, the Sun could enter the first point of Cancer on one meridian only. If, however, the year has exactly $365\frac{1}{4}$ days, there will be four equidistant points on the tropical circle which can serve as the first point of Cancer. In four years the Sun would have entered the first point of Cancer in each of the four points and the cycle would then repeat *ad infinitum*. Similar analyses could be made for the moon and planets.

Where three or more motions are simultaneously involved in the movement of a single mobile they must be treated two at a time, just as earlier propositions about conjunctions where three or more mobiles were involved.

Almost the whole of the remainder of Proposition XXII is concerned with an interesting discussion about the pattern of motion resulting from the diurnal and annual motions of the Sun in opposite directions. ORESME asserts that the center of the Sun would trace out a finite line but lacking terminating points would not be circular (carens punctis terminantibus ad modum linee circularis).[42] Rather it would trace a path forming a series of spiral lines moving from the tropic of Cancer to the tropic of Capricorn and back again.[43] There would be as many

is not a unique point but can be any point on circle *a* which happens to meet *B* in *d*. I have therefore used A_n to signify any point which might become the first point of Cancer. ORESME speaks of *a* and *circle a* indiscriminately but his meaning seems clear.

[42] Vat. lat. 4082, f. 102r, c. 2.

[43] The ultimate, and possibly immediate, source of ORESME's spiral is a passage in PLATO's *Timaeus*, 39A, B. PLATO mentions, briefly, that a spiral would result from the two oppositely directed motions of any planet, namely the diurnal motion and its motions around the zodiac in an opposite direction. Sir THOMAS HEATH, in

individual turns in the spiral as there are days between the Sun's departure from and return to the tropic of Cancer. In returning to the tropic of Cancer, newly formed spiral lines would interest those which had been traced in the downward spiral to the tropic of Capricorn.

The proposition concludes with a discussion of the Great Year. ORESME holds that a Great Year can apply to one mobile with several simultaneous motions as well as to two or more celestial bodies.[44] A Great Year is equivalent to a period of revolution for either two or more mobiles or to the several motions of a single mobile. For all the planets and the sphere of the fixed stars, ORESME believes the period will be much greater than 36,000 years, the period which some say

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his Aristarchus of Samos (Oxford, 1913), p. 169, explains the passage with reference to the diagram reproduced here (p. 160).

"Suppose a planet to be at a certain moment at the point *F*. It is carried by the motion of the Same [*i.e.* the circle of the celestial equator] about the axis *GH*, round the circle *FAEB*. At the same time it has its own motion along the circle *FDEC*. After 24 hours accordingly it is not at the point *F* on the latter circle, but at a point some way from *F* on the arc *FD*. Similarly after the next 24 hours, it is at a point on *FD* further from *F*; and so on. Hence its complete motion is not in a circle on the sphere about *GH* as diameter but in a spiral described on it. After the planet has reached the point on the zodiac (as *D*) furthest from the equator it begins to approach the equator again, then crosses it, and then gets further away from it on the other side, until it reaches the point on the zodiac furthest from the equator on that side (as *C*). Consequently the spiral is included between the two small circles of the sphere which have *KD*, *CL* as diameters." I have added the bracketed phrase. HEATH's account shows that the planet never completes a full circle but falls short because of the oppositely directed motions. This explains ORESME's statement that the finite line traced out by the sun is not circular because it lacks terminating points. See also F. CORNFORD, *Plato's Cosmology* (New York, 1957), p. 112 for PLATO's remarks, and p. 114 for CORNFORD's commentary.

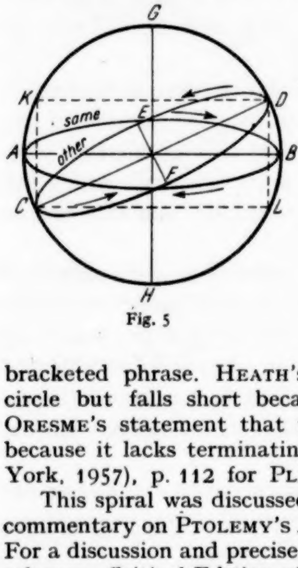

Fig. 5

This spiral was discussed by later writers such as THEON OF ALEXANDRIA, in his commentary on PTOLEMY's *Almagest*, AVERROES, AL-BITRUJI, and ALBERTUS MAGNUS. For a discussion and precise citations see FRANCIS J. CARMODY, Al-Bitruji, De motibus celorum. Critical Edition of the Latin Translation of Michael Scot (Berkeley and Los Angeles, 1952), pp. 52–54.

[44] "Unde quodlibet mobile pluribus motibus per se sumptum habet certam periodum que peracta renovatur iterum et sic infinities, et que potest vocari annus magnus istius mobilis. Consimiliter, quelibet duo mobilia celestia simul sumpta complent cursum suum certa periodo temporis, que transacta reincipiunt ut prius, et sic de tribus, sive quotlibet. Et potest dici annus magnus ipsorum, sicut dicunt quidam de sole et octava spera quod annus magnus istorum duorum est 36,000 anni solares. Sed annus magnus omnium planetarum et octave spere esset valde multo maior. Et, breviter, si omnes motus celi sint commensurabiles invicem, necesse est quod omnium simul sit una maxima periodus qua, finita, renovatur non eadem sed similis vicibus infinitis, si mundus esset eternus" (Vat. lat. 4082, f. 102v, c. 1). The concept of a Great Year was mentioned by PLATO (*Timaeus* 39D) and may antedate him. Attempts have been made to attribute to PLATO a Great Year of 36,000 years for all the planets and the celestial sphere (see HEATH, Aristarchus of Samos, pp. 171–172 and WILLIAM H. STAHL (translator), Macrobius, Commentary on the Dream of Scipio (New York, 1952) p. 221, note 3).

applies to the Sun and the sphere of the fixed stars. However this may be, there will definitely be a Great Year—or more accurately Great Years—if the celestial motions are commensurable but the Great Years will not be identical since they will occur in infinite different places.[45]

Proposition XXIII

Si aliqua mobilia talia nunc sunt coniuncta semper distabunt commensurabiliter a puncto coniunctionis et inter se.

If such mobiles should be in conjunction now, they will always be commensurably distant from that point of conjunction and from each other.

In Proposition XXIII ORESME shows that if three mobiles are presently in conjunction their respective angular distances from the point of conjunction will always be mutually commensurable as will the central angles formed by radii drawn from each mobile to the center.

If each distance is measured by a central angle which subtends an arc from the point of conjunction to each mobile, then all the angles are commensurable. Or, if we simply take the three central angles formed by the three mobiles these will also be commensurable.

The basis for the proof lies in the assumption that the mobiles have commensurable angular velocities, and therefore in any time whatever the angles and arcal distances swept out by the mobiles will be commensurable.

Proposition XXIV

Si tria nunc sunt coniuncta, quandoque duo eorum precise erunt coniuncta tertium distabit ab ipsis secundum angulum commensurabilem recto, sive per [portionem] commensurabilem toti circulo.

If three mobiles should be in conjunction now, then whenever two of these will be exactly in conjunction, the third will be distant from them by an angle which is commensurable to a right angle, or by a sector of the circle commensurable to the whole circle.

This is a variation of the preceding proposition. If three mobiles are now in conjunction in a certain point, then when any two of them will be in conjunction in some other point, the third mobile will be distant from the other two by an angle which is commensurable to the whole circle.

The places or points in which any two of these mobiles can conjunct are equidistant (by Proposition X) and divide the circle into equal segments with equal

[45] The last line in the Latin passage cited in the preceding note—"qua, finita, renovatur non eadem sed similis vicibus infinitis, si mundus esset eternus"—is unclear but ORESME seems to mean that if the world were eternal, with no beginning or end, a Great Year would begin and end in every successive position which the planets and eighth sphere occupy. The period would, however, be the same for every position. CORNFORD (Plato's Cosmology, p. 117) with reference to MACROBIUS' Somnium Scipionis, says that "since the celestial clock was never set going at any moment of time, there was never any original position to serve as starting-point. The period, whatever it may be, is beginning and ending at every moment of time." This is clearly MACROBIUS' intent for he says, "Then, just as we assume a solar year to be not only the period from the calends of January to the calends of January but from the second day of January to the second day of January or from any day of any month to the same day in the following year, so the world-year [*i.e.* Great Year] begins when anyone chooses to have it begin, ..." (STAHL (tr.) Macrobius, p. 221). The brackets are mine. ORESME may have followed MACROBIUS since his statement seems to reflect the latter's remark.

central angles. Therefore, any one of these angles taken a certain integral number of times will equal the whole surface of the circle, namely four right angles. It follows that any one of these angles is also commensurable to a right angle.

Now when any two of the three mobiles conjunct in a point which is distant from the point where the three mobiles were in conjunction by a central angle equal to k, k must be commensurable to a right angle. But in the preceding proposition it was shown that after conjunction of the three mobiles the angles separating them are mutually commensurable. Hence the angular distance separating the two mobiles in conjunction from the third mobile must be commensurable to angle k, and consequently to a right angle. Though not made explicit, it is obviously also commensurable to the whole circle.

Proposition XXV

Que proportiones motuum possint per fractiones physicas adequari, quibus scilicet utuntur astrologi sive punctualiter tabularii, et que non assignare.

[How] to determine which proportions of motions can be compared by means of physical fractions, namely those which astronomers use or punctually tabulate, and those which cannot be so compared.

The final Proposition, XXV, of part one is essentially an application of Proposition III to some of the subsequent propositions.

From Propositions X, XI, XVII, XIX, and XXI, it is clear that with commensurable motions there will be a certain fixed number of conjunctions or aspects. The whole circle can be divided into as many equal parts as there are fixed points of conjunction (Proposition X). But if, after having divided the circle into equal parts, we divide the circle again into equal parts such that none of the points of the second division coincide with any of the fixed points of conjunction, it would be impossible to determine the fixed points of conjunction by means of the second division, no matter how minutely the circle is divided in that second division.

For example, if there are seven fixed points by which the sun could enter the first point of Cancer (see Proposition XXII), or any other sign, these points could not be found by use of the common astronomical tables based on division of 60. Indeed, if the circle were divided into parts equal to $1/7^n$, where $n = 1, 2, 3, \ldots$, there would be no points in common with a division by 60 because the term immediately following 1 in the division by 7, namely 7, is prime to 60 (see Proposition II).

The astronomer, however, does not expect precise punctual knowledge of a conjunction. He is satisfied if he can determine that a conjunction occurs within a particular degree, minute, or second; or he is content if his error is not detectable by sight with some instrument.[46]

Part II. Incommensurable velocities

As mentioned previously, the velocities of the mobiles in Part II are assumed to be mutually incommensurable.

[46] "Sufficit, tamen, astrologo quod coniunctio sit in tali gradu, vel in tali minuto, vel secundo, et tunc licet ignoret in quo puncto illius minuti. Aut sufficit quod error ipsius astrologi non deprehendatur per visium cum aliquo instrumento" (Vat. lat. 4082, f. 102v, c. 2).

Proposition I

Si duo talia mobilia incommensurabiliter mota, nunc sint coniuncta numquam alias in puncto eodem coniungentur.

If two such mobiles have been moved incommensurably and should now conjunct, they will never conjunct in the same point at other times.

By means of an indirect proof, ORESME, in the first proposition shows that two mobiles now in conjunction at some point will never conjunct there again if they are moved incommensurably.

Let mobiles A and B be in conjunction in point d. In order to conjunct again in d each mobile must complete, in the same time interval, an integral number of circulations. If A makes e and B g circulations respectively, we have $S_a/S_b = e/g$ where S is distance and e and g relate the distances numerically. But when $T_a = T_b$, the velocities are related as the distances so that $V_a/V_b = S_a/S_b$ and consequently $V_a/V_b = e/g$. Since e and g are integers, the velocities must be commensurable and this is contrary to the assumption that they are incommensurable. They will, therefore, not conjunct in d.

Similarly, one can demonstrate that A and B were never in conjunction in d prior to their present conjunction. Since it is obvious that A and B conjunct in d only once, their period of revolution may be said to be infinite, although it seems more accurate to say that they have no period at all.[47]

This proof applies to any astronomical aspect where the motions are incommensurable.

Proposition II

Si duo sint nunc coniuncta, numquam alias coniungentur in puncto distanti a puncto in quo sunt per partem circuli commensurabilem suo toti.

If two mobiles should now be in conjunction, they will never conjunct in a point that is separated from the point in which they are now by part of the circle commensurable to the whole circle.

In Proposition II, ORESME demonstrates that if two mobiles moving incommensurably are in conjunction in some point, they will never conjunct in another point whose distance from the first is commensurable to the whole circle. For if the mobiles did conjunct in such a point they would have traversed a certain number of circulations plus a part of the circle commensurable to the whole. But in so doing they would have traversed commensurable distances, and consequently have moved with commensurable velocities. This, again, is contrary to the supposition that their velocities are incommensurable. We can apply this to any aspect and to the past as well as the future.

It follows from all this that the distance between proximate points of conjunction is incommensurable, and that the times between two such proximate conjunctions are incommensurable.

Proposition III

Numquam, altero eorum existente in puncto in quo nunc sunt, ambo ipsa distabunt per partem circulo commensurabilem.

When one of the two mobiles is on the point in which they are now, both mobiles will never be separated by a part [of the circle] commensurable to the [whole] circle.

Should one of the two mobiles occupy the point in which they were once in conjunction, the distance, or sector of the circle which now separates them will not

[47] "Est igitur revolutio eorum infinita, et verius loquendo nulla est" (Vat. lat. 4082, f. 103r, c. 1).

be commensurable to the whole circle. If A and B were once in conjunction in d, it follows that when A arrives again at d after some integral number of circulations, B, since it can never again conjunct with A in d, will have traversed some part of the circle incommensurable to the whole circle. For if not, B would have traversed a total distance commensurable to the distance traveled by A, which is contrary to the assumption that their velocities are incommensurable. Therefore, A and B are separated by a part of the circle incommensurable to the whole circle and they form a central angle which must also be incommensurable to a right angle and to any aspect (such as sextile, quartile, trinal) commensurable to a right angle.

Finally, these mobiles never have been, nor will be, in conjunction in any points separated by a distance commensurable to the whole circle.

Proposition IV

Nulla est circuli tam parva portio in qua talia duo mobilia non coniungantur in posterum et in qua non fuerunt aliquando coniuncta.

There is no sector of a circle so small that two such mobiles would not conjunct in it at some future time, and in which they did not formerly conjunct.

Devoting further attention to part of the circle, ORESME, in Proposition IV, demonstrates that there is no part, or sector of a circle so small that two mobiles moving incommensurably have not been in conjunction there in the past, and will not conjunct there in the future.

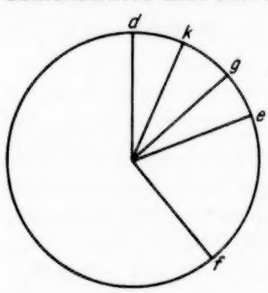

Fig. 6[48]

Let two mobiles, A and B, be in conjunction in point d, and suppose that e is the first point of conjunction after departure of A and B from d. Since the motions of A and B are respectively uniform, it follows that between any two successive conjunctions equal time intervals elapse and equal distances are traversed. The arcal distance between the points of any two successive conjunctions will, therefore, be equal.

In Fig. 6, arc de represents the distance between the first and second points of conjunction, and arc ef the distance between the second and third points of conjunction where arc de = arc ef. From what has been said in the preceding paragraph any two successive points of conjunction will be separated by arcs equal to arc de and the successive conjunctions will occur after equal intervals of time.

Now by Proposition II, Part II, arc de must be incommensurable to the whole circle because the velocities of A and B are incommensurable. As successive conjunctions occur a series of arcs all equal to de will be laid off around the circle beginning from d. We may represent this sequence of equal arcs by de_1, de_2, de_3, de_4, ..., de_n. These successive arcs cannot terminate in d because arc de is incommensurable to the whole circle,[49] and consequently after as many arcs have been marked off as can be accomodated in the first sequence of conjunctions around the circle, some arc, say de_5, will lap the circle and cross beyond d into de_1 terminating at some point g.

[48] This figure is added for convenience and does not appear in the manuscripts.

[49] Another reason they could not terminate in d is that two conjunctions can never occur twice in the same point when the velocities are incommensurable (Prop. I, Part II).

Continuing from point g a second time around the circle, further conjunctions will mark off another sequence of arcs equal to de. But now all of the previous arcs of the first division will be subdivided into smaller arcs none of which exceeds the greater of the two arcs into which de_1 has been divided at g. Thus if g should be more than half way through de_1 arc $dg >$ arc ge; but if less than halfway arc $dg <$ arc ge.[50]

Upon completion of the second sequence of conjunctions around the circle some arc, say de_{10}, will terminate in de_1 at point k, where arc $dk <$ arc dg. Assuming that arc $dk >$ arc kg, no arc of the circle will exceed ard dk after the entire circle has been divided into arcs equal to de for the third time around beginning this time, however, from point k.

Since this process can be continued *ad infinitum* and assuming an eternity of motion in the past, there would be an infinite number of points of conjunction dividing the circle into infinitely small arcs, and yet any two consecutive conjunctions are separated by an arc equal to de. All this shows that however small the arcs of the circle become they can never become so small that mobiles A and B could not conjunct there. Mobiles A and B will be in conjunction an infinite number of times but never twice in the same point.[51]

ORESME says next that if we connect the points of conjunction in their order of occurrence—*i.e.*, connect points 1 and 2 of arc de_1, points 2 and 3 of arc de_2, points 3 and 4 of arc de_3, and so on around the circle *ad infinitum* linking the successive arcs $de_1 \ldots \infty$—, we shall have an infinite number of equal angles mutually intersecting.[52] Any one of these angles will be incommensurable to a right

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[50] ORESME does not consider both alternatives, but supposes arbitrarily that no arc will exceed arc dg after all the arcs equal to de have been marked off the second time around. This is true, however, only if arc $dg >$ arc ge.

[51] "Ergo nulla erit tam parva portio quin aliquando coniungatur in futurum in aliquo puncto illius quod est propositum, et ita de preterito eternitate motuum. Unde in tali circulo coniungentur A et B infinities et semper in novo puncto per primam conclusionem huius partis, et equaliter distabit secundus punctus a primo et tertius a secundo, et sic de aliis" (Vat. lat. 4082, f. 103v, c. 1).

[52] "Et protrahendo lineam de primo ad secundum, et de secundo ad tertium, et sic deinceps describeretur in circulo una figura infinitorum angulorum equalium se invicem secantium. Et quilibet talis angulus erit incommensurabilis recto, ut faciliter probaretur. Ergo nulla pars circuli carebit in perpetuum istis angulis sed inter quecumque puncta circumferentie fierent infiniti tales anguli ita quod per equalitatem istorum angulorum, et equedistantiam ipsorum, et multiplicationem eorum in infinitum posset demonstrari de quacumque parte circuli ..." (Vat. lat. 4082, f. 103v, c. 1). The Vatican manuscript has two figures which apply to parts of Proposition IV. One of them (bottom of f. 103v, c. 1) seems appropriate to the present passage but appears incomplete and Figure 7 has been expanded to remedy the deficiencies.

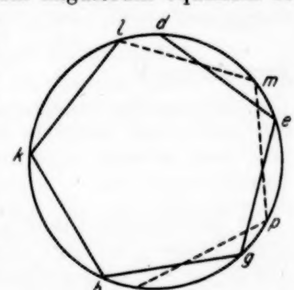

Fig. 7

By linking the successive and equidistant points of conjunction a series of equal angles is produced. Thus if the points of conjunction in successive order of occurrence are d, e, g, h, k, l, m, p, and so forth, then angles *deg, egh, ghk, hkl, klm, lmp, are* equal, and so *ad infinitum*. The phrase "infinite number of equal angles mutually intersecting" seems to apply to the legs of the equal angles criss-crossing in the figure.

angle[53] and between any two points of the circle there will be an infinite number of such equal angles so that no part of the circle will be without them.

ORESME expresses amazement and wonder at the paradoxical nature of the results derived from motions which are both incommensurable and yet regular or uniform.[54] From such a combination—incommensurability and regularity—propositions are deduced which permit us to utter remarks such as "rational irrationality," "regular difformity," "uniform disparity," and "harmonious discord." He was apparently deeply impressed by the fact that incommensurable but regular motions could produce successive conjunctions at equal time and distance intervals, and also create a complete lack of order in the sense that no conjunctions could ever occur twice in the same point, and where all of the angles are incommensurable to the circle as a whole.

But he adds what must have been the most striking paradox when he goes on to say that in dividing the circle repeatedly by equal arcs (*i.e.*, arc *de*) representing successive conjunction points, no part of the circle would remain undivided if we suppose the motions to have continued through an eternity of time in the past. Looking to the future, it is equally true that no part of the circle will remain undivided if the regular motions of the mobiles continue through an infinite future time. *And yet* no point of conjunction which served as a division point in the past can serve as one in the future because conjunctions will never occur twice in the same point. But if because of the infinite number of past conjunctions, (1) no part of the circle remained undivided in the past, *and* (2) no conjunction can occur twice in the same point, how can there be an infinite number of points in which conjunctions must occur in the future since this implies that there are still an infinite number of parts which are undivided by any point of conjunction. But this seems to contradict the assertion that no parts of the circle remained undivided in the infinite past. This is the paradox and it depends upon assuming two infinite times, past and future, separated by the present.

ORESME then adds a third infinite set of points which includes all those points removed from any of the points of conjunction by angular distances commensurable to the whole circle. In the second conclusion, it was demonstrated that conjunctions cannot occur in such points. There must, however, be an infinite number of them.

Proposition V

Quolibet puncto coniunctionis dato, in infinitum prope punctum huiusmodi mobilia coniungentur et in infinitum prope fuerunt coniuncta.

With respect to any given point of conjunction, mobiles will conjunct infinitely close to that point, and already did conjunct infinitely near that point.

[53] See preceding note for the text. After a digression outlined in the next paragraph (above), ORESME concludes Proposition IV with a proof of the assertion that any of these inscribed angles will be incommensurable to a right angle. He specifically demonstrates that angle *deg* (see Figure 7) is incommensurable to a right angle starting from the proof in Proposition II, Part II where it was shown tha. angle *doe* must be incommensurable to the whole circle.

[54] "Diligens theologus spectare potest modum mirabilem quo ex incommensurabilitate et regularitate motuum oritur quedam ut ita dicam rationalis irrationalitas, regularis difformitas, uniformis disparitas, discordia concors" (Vat. lat. 4082, f. 103v, c. 1).

Relying on Proposition IV, ORESME demonstrates in Proposition V that for any given point of conjunction the mobiles "will be, and have been in conjunction" infinitely close to that point.

If *d* is the given point, and *c* is a nearby point of conjunction, there will be a conjunction between them by Proposition IV. And if some point *f* is assigned halfway between *d* and *c*, there will be conjunctions between *d* and *f*, and so on infinitely.

The phrase "will be, and have been, in conjunction ..." is based on the discussion in Proposition IV where past and future infinites were distinctly separated.

Proposition VI

Possibile est tria, vel plura, nunc coniuncta alias coniungi quorum quodlibet respectu cuiuslibet movetur incommensurabiliter.	It is possible that three or more mobiles, with mutually incommensurable motions, are now in conjunction and will conjunct again at other times.

ORESME now moves to a consideration of three or more mobiles each of which is moved incommensurably with respect to the others. It is possible that three such mobiles now in conjunction, could conjunct again in some other point.

Assume that mobiles *A*, *B*, and *C* are in point *d*. By taking two mobiles at a time, say *A* and *B*, it can be shown that they must conjunct, at a later time, in some other point *e*, since their velocities, though uniform, are unequal. Now arc *de* must be incommensurable to the whole circle by Proposition II, Part II. Before *A* and *B* will conjunct in *e*, each will have completed a certain number of circulations with respect to *d* plus arc *de*. For example, if *A* should complete 7 circulations and *B* 5 with respect to *d*, there must then be added to each number of circulations arc *de* so the mobiles will conjunct in *e*. But arc *de* is incommensurable to the whole circle and when added to 7 and 5 circulations, respectively, will make these distances mutually incommensurable. In the same way *B* and *C* could leave *d* and conjunct in *e* since *C* might make 3 circulations plus arc *de* while *B* makes 5 plus arc *de*. Should this happen all three mobiles will simultaneously conjunct in *e*.

Indeed, though unexpressed in this proposition, ORESME holds that these mobiles will conjunct in an infinite number of different points. This emerges in Proposition IX, Part II, where ORESME cites Proposition VI as support for the contention that three mobiles can conjunct an infinite number of times[55] which is equivalent to asserting that it occurs in an infinite number of different points.

Proposition VII

Possibile est quod sint tria aut plura nunc coniuncta quorum quilibet motus sunt incommensurabiles que numquam poterunt alia vice coniungi.	It is possible that there be three or more mobiles with mutually incommensurable motions which are now in conjunction but can never conjunct in another place.

In this proposition conditions are assumed which would produce the negation of Proposition VI. That is, three or more mobiles moving with mutually incom-

[55] Referring to three mobiles, ORESME says "quod autem infinities possint coniungi patet ... de motis incommensurabiliter per sextam huius" (Vat. lat. 4082, f. 104r, c. 2). See also Proposition VII, Part II.

mensurable velocities and now in conjunction in some point will *never* conjunct in any other point. ORESME remarks, in effect, that by the assumption of one set of specific mutually incommensurable velocities it will follow that the mobiles would conjunct an infinite number of times, while from another set they will not conjunct an infinite number of times[56]—indeed they would only conjunct once.

In his demonstration of Proposition VII, ORESME relies on concepts established in Proposition II, Part I, where different possible modes of division of a continuum were discussed. It was shown that it is possible to divide a continuum according to different proportionalities, such that no point—except the first—serves as a point of division in another. ORESME *then* confined his attention to rational proportionalities, but *now* he concentrates on a continuum divided by irrational proportionalities.

As with rational proportionalities, there are some irrational proportionalities which do share common points, and others which do not. As an example of irrational proportionalities which share common points, ORESME cites $(2^{\frac{1}{2}}/1)^n$ and $(8^{\frac{1}{2}}/1)^n$ where $n=1, 2, 3, \ldots$, and only the points represented by $(2^{\frac{1}{2}}/1)^{3n}$ and $(8^{\frac{1}{2}}/1)^n$ are common to both proportionalities. Two proportionalities which do not share any common points, except the initial point represented by 1, are $(2^{\frac{1}{2}}/1)^n$ and $(3^{\frac{1}{2}}/1)^n$.[57]

[56] "... ita quod ex quadam incommensurabilitate sequitur ipsa infinities coniungi, et ex alia non sequitur" (Vat. lat. 4082, f. 104r, c. 1).

[57] After briefly summarizing the key points of Proposition II, Part I, namely that a continuum could be divided in one way by rational proportionalities which share common points, and in another by proportionalities which do not share common points, ORESME goes on to apply this to irrational proportionalities. He says "si proportionalitates essent communicantes [the reference is to rational proportionalities] eodem modo necesse est esse de proportionalitatibus secundum proportiones irrationales. Si enim una sit secundum medietatem duple, et alia secundum medietatem octuple, tunc sunt communicantes. Sed si una fiat secundum medietatem duple, et alia secundum medietatem triple, tunc sunt incommunicantes" (Vat. lat. 4082, f. 104r, c. 1).

In medieval mathematical terminology *medietas duple* often means—as it clearly does here—$(\frac{2}{1})^{\frac{1}{2}}$, *medietas octuple*, $(\frac{8}{1})^{\frac{1}{2}}$, and so forth. The above passage must be interpreted analogically to the division by rational proportionalities in Proposition II, Part I. Thus to divide a given rectilinear continuum according to a proportionality of *medietas duple*, that is $(2^{\frac{1}{2}}/1)^n$, we divide the continuum successively into $2^{\frac{1}{2}}$ equal parts, 2 equal parts (*i.e.* $2^{\frac{2}{2}}$), $2^{\frac{3}{2}}$ equal parts, 4 equal parts (*i.e.* $2^{\frac{4}{2}}$), and so on. The parts will, of course, become smaller and smaller. By ORESME's special use of the term *commensurabilis*, which is found in his earlier treatise *De proportionibus proportionum*, all the parts of the successive divisions will be equal and commensurable (see Footnote 60 below) because they are in the same geometric series and have rational exponents.

Now when the same continuum is divided by the other irrational geometric proportionality $(8^{\frac{1}{2}}/1)^n$, there will be common points. For example, when it is divided into $2^{\frac{6}{2}}$ and $8^{\frac{2}{2}}$ equal parts all the points coincide for each divides into eight equal parts. In the two proportionalities selected as illustrations by ORESME, there are common points only where particular terms of each of the proportionalities can be expanded to rational numbers.

In the second case, the successive divisions of the two irrational proportionalities share no common points because the base terms, 2 and 3, are prime to each other. This holds even when particular terms of the respective proportionalities are expanded to rational numbers.

ORESME turns next to considering a circular continuum where two such series of infinite numbers are incommensurably distant but will never share any common points. Let us suppose that there are three mobiles, A, B, and C which are now in conjunction in point d (Fig. 8[58]). Subsequent points of conjunction for A and B will be in points e and f. Points e and f, says ORESME, must be equidistant because, as always, the motions of the mobiles, though different, are respectively uniform. Hence arc de equals arc ef. Finally, ORESME sets the ratio of the whole circle to arc de as $(3:2)^{1/2}/1$.[59]

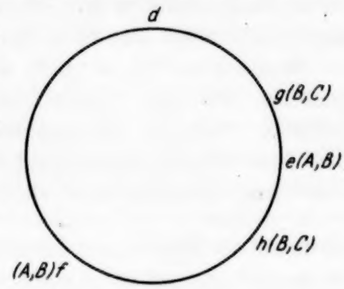

Fig. 8[58]

Mobiles B and C, after departing from d, will conjunct next in g, and then h, and so on, where arc dg equals arc gh and, indeed, all of the arcs formed by the successive conjunctions of B and C equal arc dg. The ratio of the whole circle to arc dg is $(4:3)^{1/2}/1$.

These two ratios, namely $(\frac{4}{3})^{\frac{1}{2}}$ and $(\frac{3}{2})^{\frac{1}{2}}$, are incommensurable irrational proportions, which ORESME says he has already shown in his treatise *De proportionibus*

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[58] This figure appears in Vat. lat. 4082, f. 103v, c. 2, lower margin.

[59] The text setting out the data for this portion of the proposition is as follows: "Verbi gratia, posito quod A et B, que nunc sunt in d, postea coniungentur in e deinde f, tunc $d\ e\ f$ etiam equaliter distant per regularitatem motuum. Sitque totus circulus ad arcum de in mediate proportionis sexquialtere" (Vat. lat. 4082, f. 104r, c. 1). As A and B move through successive conjunctions the distance separating any two successive conjunctions equals arc de which, in turn, equals a $1/(3:2)^{\frac{1}{2}}$ part of the entire circle. Moreover, this is just what ORESME means by dividing a circle according to some irrational proportionality. Conjunctions continue to occur *ad infinitum* and as the mobiles move round and round they leave a never ending sequence of equally spaced points. As this continues the points crowd together but any two successive conjunctions are equidistant and in this sense the circle is divided into equal parts. Now this is analogous to dividing a finite rectilinear continuum into as many equal parts as any *one* of the terms of the given proportionality. For example, if the proportionality is $(\frac{3}{1})^n$ and $n=3$, the continuum can be divided into twenty-seven equal parts, and the immediate task of division is then completed, although, of course, it may be continued *ad infinitum*, by making $n=4$, then 5 and so on.

There are, however, important differences not mentioned—and perhaps undetected—by ORESME. The division of a circular continuum is, as we have seen, never completed if the motions are incommensurable, whereas in a rectilinear continuum even if the proportionality were irrational the continuum could be theoretically completed in the manner discussed in Footnote 57, and in the preceding paragraph.

Another difference is that every successive term in any geometric proportionality will succeed in dividing a rectilinear continuum into a greater number of smaller equal parts. But in a circular continuum not every successive term of every geometric proportionality is capable of dividing the circle into equal parts (in the sense described above in this note) without violating previous propositions. Using the example above, the geometric series is $[(3:2)^{\frac{1}{2}}/1]^n$, and when $n=1$ a situation described in the first paragraph of this note obtains. But when $n=2$ the ratio of the whole circle to the distance separating any two successive conjunctions would be $3:2/1$. Therefore, the distance separating any two successive conjunctions is $\frac{2}{3}$ of a circle. But $\frac{2}{3}$ of a circle is commensurable to the whole circle and this is contrary to Proposition II, Part II, where it is demonstrated that any two mobiles moving incommensurably cannot conjunct in any point removed from a previous point of conjunction by a distance com-

proportionum.[60] Consequently, no point, except *d*, can be a common point of conjunction for *A*, *B*, and *C*, since all points of conjunction for *A* and *B* will differ from those for *B* and *C*. The three mobiles have not been, and never will be in conjunction in any other point except *d* where they conjuncted only once and this is what was to be demonstrated.

Proposition VII is really an extension of Proposition IV from two to three mobiles. The conditions established for a pair of mobiles in the latter proposition were that between any two successive points of conjunction the arcs were equal, and each was incommensurable to the whole circle. In Proposition VII the same conditions are applied to three mobiles sorted into two pairs.

Proposition VIII

Si fuerint tria vel plura nunc coniuncta que omnia commensurabiliter moveantur preter unum cuius motus sit aliis incommensurabilis, numquam alias coniungentur nec alia vice fuerunt coniuncta.

If three or more mobiles should now conjunct and they are moved commensurably, except one whose motion is incommensurable to the others, they will never conjunct at other times nor did they conjunct in another place.

Once again mobiles *A*, *B*, and *C* are in conjunction in *d*, but in Proposition VIII *A* and *B* are taken to move with commensurable velocities, and *B* and *C* incommensurably. From this data, ORESME demonstrates that *A*, *B*, and *C* will never conjunct, and have never been in conjunction in any other point.

Since *A* and *B* are moved commensurably it follows from a corollary of Proposition X, Part I, that any point in which *A* and *B* conjunct must be removed from point *d* by a distance commensurable to the whole circle.[61] But *B* and *C*, on the other hand, move with mutually incommensurable velocities and are removed from *d* by a distance incommensurable to the whole circle (Proposition II, Part II). Therefore *A*, *B*, and *C* cannot have had, nor could they have, any common point of conjunction other than point *d*.

Proposition IX

Omnia tria aut plura mobilia aut numquam simul, aut semel solum, aut infinities toto eterno tempore coniungentur.

Any three or more mobiles will in an eternal time either never conjunct, conjunct only once, or conjunct an infinite number of times.

Drawing on a number of earlier propositions, ORESME shows in Proposition IX, that in all cases where three or more mobiles are moving commensurably or

mensurable to the whole circle. Presumably, in order to avoid this dilemma, ORESME would have to select an irrational proportionality with an irrational exponent. This would prevent any rational numbers from appearing in the series when any of the terms were expanded. For example, $[(3:2)^q/1]^n$, where q is irrational and n is the sequence of natural numbers.

[60] See p. 305, 306, of my article cited in Footnote 1. ORESME distinguished between irrational proportions which were mutually commensurable and those mutually incommensurable. Any two irrational proportions which could be related by a rational exponent were commensurable, if not they were incommensurable. Thus $(\frac{4}{1})^{\frac{1}{3}}$ and $(\frac{2}{1})^{\frac{1}{2}}$ are commensurable because they can be related by the rational exponent $\frac{4}{3}$ in the form $(\frac{4}{1})^{\frac{1}{3}} = [(\frac{2}{1})^{\frac{1}{2}}]^{\frac{4}{3}}$. But in ORESME's example, $(\frac{4}{3})^{\frac{1}{2}}$ and $(\frac{3}{2})^{\frac{1}{2}}$ cannot be so related and are incommensurable.

[61] Proposition X, Part I, showed that a circle could be divided into as many equal parts as there are points of conjunction. Hence the distance from *d* of any point of conjunction must be commensurable to the whole circle.

incommensurably, there are only three possibilities for the number of conjunctions which may occur through an eternal past and future time.

On the basis of previous propositions, three mobiles will either (1) never conjunct, (2) conjunct only once, or (3) conjunct an infinite number of times. Any finite number of conjunctions commencing with two is consequently impossible.

In Proposition XII, Part I, ORESME says he demonstrated that three or more mobiles moving with commensurable motions might, under certain conditions, never conjunct. The same thing might obtain for mobiles moving incommensurably and here he cites Proposition VII, Part II.[62] Propositions VII and VIII, Part II, are cited for those cases in which only one conjunction is possible where the motions are incommensurable.[63] No propositional counterpart is cited for commensurable motions producing only one conjunction. For an infinite number of conjunctions Proposition XIV, Part I, supports the claim for commensurable motions, and Proposition VI, Part II, for incommensurable motions.

ORESME now furnishes reasons why there cannot be a finite number of conjunctions greater than one. Even if three or more mobiles, moving with commensurable velocities, should conjunct in a finite number of points in a circle, the sequence of conjunctions through an eternal past and future time will repeat itself infinitely in an identical manner, since the motions of the mobiles are respectively uniform. And if the mobiles are moved with incommensurable velocities there will also be an infinite number of conjunctions, except that unlike the case with commensurable motions, there will be an infinite number of points of conjunction but only one possible conjunction in each of them.

Proposition X

Si tria aut plura mobilia incommensurabiliter moveantur, numquam essent ita propinqua quin aliquando sint propinquiora quantumlibet in infinitum.	If three or more mobiles should be moved incommensurably, they would never be so close that they could not be ever so much closer into infinity.

The problem of how close three or more mobiles can approach short of a conjunction is dealt with in Proposition X. There, ORESME asserts that if three or more mobiles are moving incommensurably, no matter how small a space encompasses them, they can, at other times, approximate even closer to one another without moving into actual conjunction.

Let d be a point in which at one time mobiles A, B, and C have been in conjunction, and consequently will never conjunct there again. Both A and B will

[62] "Quod possibile sit ipsa numquam coniungi de commensurabiliter motis patet per 12am prime partis. Et idem patet contingere de incommensurabiliter motis quod patet etiam satis per septimam huius, quia possibile est quod puncta in quibus A et B coniungentur et puncta in quibus B et C coniungentur non communicent, nec in uno, nec in pluribus" (Vat. lat. 4082, f. 104r, c. 2). Nowhere in Proposition VII does ORESME say that there might possibly be no conjunctions whatever. In citing it as support for this contention, he seems to be inferring that this would be possible if the mobiles did not begin from conjunction. In that event, as he says in the passage quoted, they might not share one or more points.

[63] "Sed quod possint coniungi toto eterno tempore solum semel, demonstratum est per duas conclusiones immediate precedentes [*i.e.* VII and VIII]" (Vat. lat. 4082, f. 104r, c. 2).

individually move from *d* to *d* an infinite number of times and the times in which they respectively complete one circulation are incommensurable. Furthermore, since the velocities of *A* and *B* are unequal, and incommensurable, it will happen that at some time *A* will be in *d* and a short time later *B* will arrive in *d*; but at some other time *B* will arrive in *d* within a still shorter interval of time after *A* was in *d*. The time elapsing between the entry of *A*, and then *B*, into point *d* can become less and less, and *A* and *B* will come closer and closer to each other with reference to *d*. No matter how close they come, however, the intervening distance can become smaller at another time. The same may be said for *C* with reference to *A* and *B* respectively.[64]

In this way the three mobiles can approach ever closer together for no matter how small a space embraces them it can become still smaller. The ever diminishing approximation is similar to the way in which no part of the arc, in Proposition IV, Part II,[65] remains undivided through an eternal time. There, it will be recalled, it was shown that between any two points of conjunction, no matter how close, other conjunctions can take place so that the arcal distance between any two adjacent points of conjunction is continually diminished.

It is now clear that the proximity possible between mobiles, short of actual conjunction, depends on whether the motions are commensurable or incommensurable. Earlier, in Proposition XIII, Part I, it was shown that there is a minimum distance of approximation for two mobiles moving commensurably. For mobiles moving incommensurably there can be no minimum distance.

Proposition X terminates with ORESME applying the results to planetary motions. We could suppose that the motions of all, or several planets, are mutually incommensurable, but at the least let us assume that no three planets have their motions mutually commensurable. For three such planets, this condition is compatible with any two of them moving commensurably and the others incommensurably.[66] Should such conditions obtain it would follow that the planets involved could, at some time or other, occupy the same degree, and at another time the same minute, and at yet a different time the same second, and so forth. But however small the space becomes, the three or more planets will never exactly conjunct.[67]

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[64] "Ergo aliquando quando *A* erit in *d* parvum tempus deficiet quando *B* sit in *d*. Ergo, essent satis propinqua, et adhuc aliquando minus tempus postea deficiet quando *B* sit in *d*. Propter istam incommensurabilitatem, ergo, adhuc essent propinquiora, et sic in infinitum. Et eodem modo diceretur de *C* mobili respectu utriusque istorum sigillatim, et ita de quotlibet mobilibus. Ergo non erunt ita propinqua quin adhuc sint propinquiora in futurum" (Vat. lat. 4082, f. 104v, c. 1).

[65] "Et ubique per totum circulum erit approximatio eodem modo quod dictum est de coniunctione duorum mobilium in quarta conclusione huius partis" (Vat. lat. f. 104v, c. 1).

[66] As in Proposition VIII, Part II.

[67] "Posito, ergo, quod omnium, aut plurium, planetarum motus sint incommensurabiles, scilicet quilibet motus cuilibet, aut saltem quod nulli tres motus sint invicem commensurabiles quamvis essent commensurabiles bini et bini, dico ergo quod necesse est si ita sit illos planetas in eodem gradu aliquando convenire, et aliquando in eodem minuto, et quandoque in eodem secundo, et tertio, et quarto, et sic in infinitum approximando. Et tamen, numquam punctualiter coniungentur et adhuc hoc sequitur de quolibet gradu celi, minuto, secundo, tertio, quarto, et cetera" (Vat. lat. 4082, f. 104v, c. 1—c. 2).

Proposition XI

Que de coniunctionibus duorum aut plurium mobilium dicta sunt, pari ratione, intelligenda sunt de omni alio aspectu seu modo se habendi.

Those things which have been said about conjunctions of two or more mobiles must be understood to apply, by the same reasoning, to every other aspect or relationship.

Up to this point all the demonstrations in Part II have been confined exclusively to conjunctions. In Proposition XI most of the previous propositions are shown to apply as well to the other astronomical aspects. Thus the present proposition serves as the counterpart of Proposition XXI, Part I, which did the same for earlier demonstrations involving only commensurable motions.[68]

In general ORESME shows that just as no conjunction ever occurs twice in the same point, so no other aspect can occur twice in exactly the same way. Actually, but little space is devoted to this and ORESME concerns himself rather with more speculative questions. He asks why it is that by assuming incommensurable motions a certain conjunction, for example, can take place but once and yet, prior to its occurence, it was necessary that it should happen through an eternal future.[69] We cannot explain such things, nor why it should occur at some particular instant and not at another unless we attribute it to the velocities of the motions and the unchangeable inclinations of the mobiles.[70]

ORESME muses over some of the consequences flowing from the situation just described.[71] If dispositions or configurations of celestial bodies cause inferior effects, it is possible that a unique disposition might occur. Now unusual or notable configurations could affect an entire species and it is, therefore, conceivable,

[68] "... quod docet vicensima prima prime de motibus commensurabilibus, idem proponit de incommensurabilibus presens conclusio" (Vat. lat. 4082, f. 104v, c. 2).

[69] "Supposita namque incommensurabilitate motuum et eternitate pulcrum est considerare qualiter talis constellatio sicut esset coniunctio punctualis eveniet semel solum in toto tempore infinito, et quomodo ab eterno futura erat necessario pro hoc instanti nulla simili precedente aut sequente" (Vat. lat. 4082, f. 104v, c. 2). Throughout his discussion of incommensurable motions, ORESME has assumed that time is eternal. But he treats it as a two-fold infinite where the occurrence of any conjunction serves as a division point between an infinite past time and an infinite future time. Any conjunction presupposes an infinite past time, and an infinite time ago it could have been said of this particular conjunction that it would have to occur in an infinite future time. This seems to be the sense which ought to be attached to the phrase "ab eterno futura erat necessario pro hoc instanti nulla simili precedente aut sequente ...".

[70] "Nec est querenda ratio quare magis eveniret tunc quam alias, nisi quia tales sunt velocitates motuum et immutabiles voluntates moventium" (Vat. lat. 4082, f. 104v, c. 2).

[71] "Et si constellationes sint cause inferiorum effectuum continue erit talis dispositio quod numquam erit similis in hoc mundo. Cum que notabiles aspectus respiciant totam unam speciem, non videtur inopinabile, loquendo naturaliter, quod una magna coniunctio planetarum cui numquam fuit similis producat aliquod individuum cui non fuerit simile in specie ... Et forte possibile est quod talis species incepta numquam desineret si mundus perpetuaretur, aut quod aliquando desineret virtute alterius constellationis. Et sic de similibus correlariis que ex dictis possunt elici" (Vat. lat. 4082, f. 104v, c. 2—105r, c. 1).

"speaking naturally" *(loquendo naturaliter)*,[72] that a great conjunction of planets which could occur only once might produce a unique species unlike any other. Furthermore, if the world were eternal this new species might never cease to exist, or it might cease to exist by virtue of some other configuration which would cause it to go out of existence. One might draw other corollaries from the previous propositions on incommensurable motions.

Proposition XII

De eodem mobili quod pluribus motibus movetur enunciare consimilia prius dictis.

[How] to apply to one and the same mobile moved with several [simultaneous] motions propositions similar to those which have been previously enunciated.

The twelfth, and final, proposition of Part II is perhaps the most interesting. It is the direct counterpart of Proposition XXII, Part I, where one mobile, the sun, was assigned two simultaneous commensurable motions. In the present proposition the sun is assigned two simultaneous but incommensurable motions—diurnal and annual.

Let A_n, where $n = 1, 2, 3, \ldots \infty$, be the first point of Cancer. Any such point will describe a complete circle daily as it is carried round by the tropic of Cancer. The center of the sun, B, traverses the ecliptic in a solar year. As in Proposition XXII, Part I, imagine that A_1 and B are in conjunction at point d fixed in space. ORESME then invokes a number of earlier propositions in Part II concerned with two or more mobiles.

By Proposition I it can be shown that A_1 and B now in conjunction at d can never again conjunct there.[73] Hence if the sun should enter the first point of Cancer on some particular meridian it could never do so again, nor could it have done so in the past. Furthermore, by Proposition II, the sun can never enter the first point of Cancer on any meridian which is distant from the first meridian (at point A_1) by a distance commensurable to the whole circle.[74]

There are also an infinite number of points on the tropical circle on which B entered Cancer in the past, and there will be infinite others on which B will enter Cancer through an infinite future. Indeed, there is no arc or sector of the circle so small that it does not contain some meridian on which B has, in the past, entered and on which B at some future time will enter the first point of Cancer. This applies to any point of the Zodiac and all this can be demonstrated in the same manner as Proposition IV.[75]

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[72] By "loquendo naturaliter" ORESME presumably means in accordance with natural philosophy, in contrast to speaking according to faith or dogma. Thus, for example, it would be permissible to speak of the motion of the sun through an infinite past and future time when speaking "naturaliter", but not permissible according to faith since the import of this would be that the sun was eternal and hence uncreated.

[73] "Dico igitur quod A et B numquam erunt simul alias in puncto d quod probabitur omnino similiter sicut probata est prima conclusio" (Vat. lat. 4082, f. 105r, c. 1).

[74] "Nec etiam ipso existente in meridiano distanti ab isto commensurabiliter ut probatur per secundam huius" (Vat. lat. 4082, f. 105r, c. 1).

[75] "Sint quoque infinita puncta in isto circulo *a*, ubique dispersa, in quorum quolibet B existens intravit cancrum, et alia infinita in quorum quolibet B existens

Once again, as in Proposition XXII, Part I, ORESME turns to a consideration of the "Platonic spiral." In contrast to the spiral traced by the sun when it was assumed to be moving with two commensurable motions, the resultant spiral from two incommensurable motions has no beginning and no termination. Every day through an infinite past it has swept out a new spiral which is never again retraced. And through an infinite future it will describe an infinite number of new spirals.[76]

ORESME provides little additional information but says it all follows from previous remarks in this very proposition. It seems that the infinite spiral results from the fact that the sun enters the tropic of Cancer always at a different point and proceeds to spiral down toward the tropic of Capricorn (see Figure 5) which it enters always at a different point. It then spirals upward, once again, to the tropic of Cancer. Thus the spiral never terminates since the sun arrives and departs from a different point at both of the tropical circles and could never touch the same point twice on either circle. Furthermore, since the sun always commences its motion toward the tropic of Capricorn from a new point on the circle of Cancer, it could never retrace a previously formed spiral within the fixed space in which ORESME imagines the spirals to be described.

Now as the sun moves down in its annual motion from Cancer to Capricorn it sweeps out one spiral at the completion of each daily motion and will intersect a point on each of these spirals as it moves upward from Capricorn to Cancer. It can be said, therefore, that *B*, the center of the sun, has been in every one of these points of intersection twice through all eternity, since all previous and subsequent annual spiral paths will differ and in each a unique set of points has been intersected. But apart from these points of intersection, all other points may be ranged in two classes—those through which *B* will never pass, and those through which it passes only once.[77]

This eternal motion through the vast expanse between the tropical circles can be conceived to form a large criss-cross pattern, or net-like structure. Since the spirals have, in the past, been infinite in number, the spiral lines must have

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intrabit in posterum idem signum ita quod nulla est huius circuli tam parva portio in qua non sit aliquis meridianus talis quod *B* existente in eo ipsum *B* erat in primo puncto cancri, et in qua non sit aliquis meridianus talis quod *B* aliquando existens in eo erit in primo puncto cancri, sicut de coniunctione dictum est in quarta conclusione huius. Et sicut dictum est de primo puncto cancri, ita intelligendum est de quolibet puncto zodiaci" (Vat. lat. 4082, f. 105r, c. 1).

[76] "Ex quo sequitur quod omni die *B* describit unam novam spiram in spatio ymaginato immobili quam numquam alias descripsit et viam percurrit quam numquam alias peragravit. Et sic suo vestigio, seu ymaginato, fluxu prolongare videtur lineam girativam iam infinitam ex infinitis spiris in preterito descriptis confectam quandoque des tropico ad tropicum quasdam spiras describit. Et iterum revertendo que priores intersecant, et econverso" (Vat. lat. 4082, f. 105r, c. 1).

[77] "Et ergo in quolibet puncto harum intersectionum bis existit *B* in toto eterno, et in quolibet alio aut semel solum, aut numquam" (Vat. lat. 4082, f. 105r, c. 1). The class of points through which *B* passes only once consists of all those points along the infinite spiral line exclusive of the points of intersection. Points through which *B* never passes are, presumably, points on either tropical circle which can never serve as the first point of Cancer or Capricorn. For example, any point which is removed from *d* by a distance commensurable to the whole circle will never meet *B* at *d*.

formed an infinitely compressed, or thickened *(inspissata)* structure. And yet it will continue to be infinitely thickened in the future.[78]

The properties of the spirals are briefly discussed in connection with eccentric and epicyclic motion. When *B*, the sun, moves about a center other than the earth, or center of the world, it will constantly approach and recede from the center of the world, which is eccentric. Consequently, the spirals will approach and recede. The sun, in its annual motion, will pass through every meridian but on any given meridian its distance from the earth is unique for when it crosses that meridian at any other time it will always be more or less distant from the *centrum mundi.*[79]

Another consequence following from these two incommensurable solar motions is that the exact length of the solar year is impossible to determine, since the time of each diurnal rotation is incommensurable to the time the sun takes to complete a revolution around the ecliptic. This means that the solar year consists of some integral number of days plus a part of a day incommensurable to a whole day. No perpetual almanac or true calender could be established from these circumstances.[80] It is obvious that ORESME is concerned here only with the *mathematical impossibility* of arriving at a true almanac or calendar. He was certainly aware that a true calendar would be practically unattainable even if the fractional part of the day should be commensurable to a whole day. Inherently defective human sense organs would introduce errors which prevent the construction of an exact calendar.

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[78] "Secundum hanc igitur ymaginationem, totum celi spatium inter duos tropicos exaratur ab ipso *B* deliquendo ex istis girationibus figuram velut opus texture, aut rethis, per totum illud spatium expanse. Et huiusmodi textura iam tempore preterito perpetuo fuit in infinitum inspissata, et, tamen, adhuc continue inspissatur eo quod fit cotidie nova spira" (Vat. lat. 4082, f. 105r, c. 1—c. 2).

[79] "Item si moveatur secundum circulum eccentricum vel epiciclum. Ratione huius describit spiras suas approprinquando ad centrum mundi, et aliquando recedendo. Propter quod in quocumque meridiano *B* existat numquam alias erat precise tantum distans a centro mundi, ipso [i.e. *B*] existente in eodem meridiano, sed semper plus aut minus" (Vat. lat. 4082, f. 105r, c. 2).

ORESME's precise meaning in this passage is unclear. But if the spiral lines are traced out on the celestial sphere—as is likely—then it would seem ORESME has erred in asserting that for any point lying on an arc of a meridian cut-off between the two tropical circles *B* will be a unique distance away from the eccentric *centrum mundi*. In EUCLID's Elements, III, 7 demonstrates that for any eccentric point (representing, in ORESME's proposition, the eccentric *centrum mundi*) lying on the diameter of a circle two equal straight lines—and no more—can fall on the circumference of the circle. Hence the rectilinear distance from the *centrum mundi* to any point on an arc of a given meridian which lies between the equator (functioning as diameter of the meridian circle) and one of the tropical circles, is equal to the rectilinear distance of a corresponding point lying between the equator and the other tropic. The two points are the same arcal distance away, though in opposite directions, from the equator. Thus the spiral moving through an infinite time could possibly pass through two such corresponding points both equidistant from the *centrum mundi*—contrary, to ORESME's statement.

[80] "Adhuc, autem, ex predicta incommensurabilitate contingeret quod annus solaris medius contineret aliquos dies et portionem diei incommensurabilem suo toti. Que posito, impossibile est precisam anni quantitatem deprehendere, aut perpetuum almanac condere, seu verum kalendarium invenire" (Vat. lat. 4082, f. 105r, c. 2). The word "numerus" appears after "quantitatem" in the manuscripts but has been omitted here because it does not appear to fit the sentence grammatically.

In concluding Part II, ORESME observes that incommensurable motions cannot be related or equated to numbers and hence from such motions it would be impossible to produce tables for conjunctions, oppositions, or any other aspects.[81]

The topics of commensurability and incommensurability, as mentioned at the outset, seem to have utterly fascinated ORESME. It might be said, with justification, that he was especially enthralled with incommensurable relationships from which as we have seen, he could elicit all manner of paradoxes. ORESME's enthusiasm for this subject was, however, shared by few others—either before or after his time. Though he definitely influenced others (see Footnote 2) it was through his discussion of incommensurability as applied to mathematical proportionality and local motion,[82] rather than to circular motion generally, and celestial motion particularly. Indeed, though some may have mentioned the possibility of the incommensurability of the celestial motions,[83] ORESME is, at

[81] "Unde universaliter certum est quod nulli motus incommensurabiles possunt per numeros adequari, nec est possibile coniunctiones, oppositiones, et aspectus huiusmodi motuum tabulare" (Vat. lat. 4082, f. 105v, c. 1).

[82] These topics constitute chapters one through four of his De proportionibus proportionum.

[83] For example, AVERROES mentions, in the course of a discussion as to whether the nature of eternity and continuity is cyclical or rectilinear, that it would be almost impossible to determine whether the motions of the sun and moon are commensurable or not. SAMUEL KURLAND (ed. and tr.), Averroes on Aristotle's, De Generatione et Corruptione, Middle Commentary and Epitome (Cambridge, Mass., 1958), p. 138.

More positively, there is an anonymous fourteenth century treatise in which the *magnus annus* is opposed on grounds of impossibility since the month and year are incommensurable. See LYNN THORNDIKE, A History of Magic and Experimental Science (New York, 1934), vol. III, p. 582.

Of greater significance are a few cases where ORESME exerted a direct influence. HENRY OF HESSE (HEINRICH VON LANGENSTEIN), ORESME's contemporary at the University of Paris, in his Tractatus de reductione effectuum specialium mentions that ORESME has shown the impossibility of determining whether the motions and speeds of all the planets are mutually commensurable or not. See PIERRE DUHEM, Le Système du monde (Vol. VIII, Paris, 1958), p. 483. In arguing against astrological prediction, HENRY seems once again, this time without citation, to rely on ORESME, when, in his Tractatus contra astrologos coniunctionistas de eventibus futurorum, he argues that the foundations of astrology cannot be based on identically recurrent astronomical experiences since astronomical events are not of this type "propter motuum superiorum varietatem et incommensurabilitatem." For the passage see HUBERT PRUCKNER, Studien zu den astrologischen Schriften des Heinrich von Langenstein (Berlin/Leipzig, 1933) p. 159.

JEAN GERSON, in his Trilogium Astrologiae theologizatae specifically cites ORESME in support of the contention that it is wholly uncertain whether celestial motions are commensurable or not (DUHEM, *op. cit.*, vol. VIII, p. 454).

NICHOLAS OF CUSA, when considering the problem of calendar reform in his Reparatio calendarii, which he presented to the Council of Basle in 1436, argued against the possibility of an exact astronomical science and precise calendar reform on grounds that the celestial motions are incommensurable and hence impossible to denominate exactly. DUHEM insisted that CUSA was directly influenced by an earlier anonymous treatise in which CUSA's arguments may be found. This "anonymous" work is none other than ORESME's De commensurabilitate vel incommensurabilitate motuum celi. It seems that CUSA drew upon ORESME's arguments to buttress his strong scepticism concerning human ability to acquire exact knowledge. For a summary of CUSA's arguments see DUHEM, *op. cit.*, vol. X (Paris, 1959) pp. 310—313; DUHEM's discussion

present, the only one known to this writer who ever treated it mathematically and devoted at least one complete treatise to the subject.

If ORESME was the first to deem this topic worthy of precise treatment, why we may ask, did his contemporaries and those who came after fail to pursue it, or even comment upon his treatise? Does ORESME in effect supply a partial answer when, as already mentioned, he says that astronomers are content with a degree of accuracy within the limits of observational instruments? In such an event they would hardly show interest in discussions of precise punctual relations and *a fortiori* even less concerned with the effect of assumed incommensurable motions on such relations. In brief, it was irrelevant for astronomers.

Despite this other scholastics might have taken it up as an academic discussion or as an argument concerning possible celestial relations, or simply as a mathematical exercise. That this seems not to have happened may be attributable, for lack of a better reason, to the intrinsic difficulty of the subject. Few scholars in the middle ages would have been equipped to discuss it, or imaginative enough to have seen possible applications of it in a variety of contexts. Mathematical incommensurability was never destined to be a popular academic subject, and its application to circular and planetary motion even less appealing.

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of the "anonymous" treatise is found in vol. VIII, pp. 454–462, where he conjectures that perhaps PIERRE D'AILLY was its author (p. 455). That it is by ORESME is immediately obvious from the opening lines quoted by DUHEM.

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